THURSDAY, JULY 21, 1892.

DR. MIVART'S ESSAYS.

Essays and Criticisms. By St. George Mivart, F.R.S. (London: Osgood, M'Ilvaine, and Co., 1892.)

DR. MIVART has collected in two portly volumes a number of essays and critical reviews which he has from time to time contributed to current monthly or quarterly literature. The ground covered is tolerably extensive; from "Jacobinism" and "The French Revolution" to "Weismann's Theories" and "Eimer on Growth and Inheritance;" from "Austrian Monasteries" and "The Greyfriars" to "Herbert Spencer" and "Hermann Lotze." We have read the whole, or almost the whole, with interest, and not without admiration of the author's wide knowledge, his earnest purpose, and his power of clear exposition. Here, however, we are chiefly concerned with those essays which deal with scientific problems. They are well worthy of reperusal in their present collected form, and that chiefly because Dr. Mivart holds definite and in some respects peculiar views on evolution, because he has the advantage of some training in philosophy, because he is a learned and acute critic, and because he has pre-eminently the courage of his convictions.

It is scarcely necessary to remind the readers of NATURE that Dr. Mivart is one of those who hold that natural selection has played a quite subordinate part in the evolution of organisms. He believes that the concurrence of certain external exciting causes acts in such a manner on internal predisposing tendencies as to determine by direct modification the evolution of new specific forms. Furthermore he affirms that, beyond the domains of merely physical science (which, though much, is not everything), reason demands a non-mechanical conception—namely, the conception of an immanent active principle or soul in everything which lives. And he contends that between the self-conscious reason of man and the mere sensuous feeling of the higher brutes, there is a great and impassable gulf fixed. These are among the more important positions which the author of these "Essays and Criticisms" assumes in the field of biological speculation. And to these may, perhaps, be added his condemnation of the doctrine of the relativity of knowledge, and his belief in common-sense realism, apparently on the assumption that the external reality of the objective world (as opposed to its *phenomenal* existence) is directly apprehended by the intellect, though it cannot be reached through sensuous feeling.

On all these matters Dr. Mivart has much that is interesting to say, and says it in an interesting manner. It would manifestly be impossible here to discuss so wide a range of problems. We therefore propose to select one matter—that of the relation of human reason to brute intelligence—on which to offer a few remarks.

In the essay entitled "A Limit to Evolution," the author seeks to establish the impossibility of mental evolution as applied to man. He insists, and rightly insists, that the great difference between man and the lower animals lies not in his bodily but in his mental constitution; and he contends, again in our opinion with perfect justice, that in order to examine this question we must begin by looking a little carefully into our own minds, and by examining our own acts and mental nature. As the result of this examination he finds that our psychical operations fall into two classes; on the one hand, there are feeling (sensitivity), imagination and sensuous memory, sensuous emotion, sense-perception, and sensuous inference; on the other hand, there are intellectual perception, ideation and conception, abstract ideas, and moral and æsthetic concepts. "The contrast, the difference of *kind*," he says, "which exists between this *intellectual conception* and the various forms of *feeling* is very great." We thus possess a dual psychical nature, on the one side sensuous, on the other side intellectual. The sense-perceptions of the one and the abstract ideas of the other "belong to utterly different categories, and a nature which has this power of abstraction is separated from any nature which has *not* that power, by a gulf which is an impassable *limit to evolution*, because feeling and intellect are both thus different in nature, and progress and develop along different and more or less diverging roads." But the psychical powers of brutes are limited to sense-perception, and give no evidence of the possession of the higher faculty of ideation and conception. Therefore the passage from the so-called mind of the brute to the conceptual mind of man is not only impossible but inconceivable.

Such in brief is Dr. Mivart's line of argument. Now, we hold that the distinction between the higher self-conscious, reflective, and conceptual powers of man, and his lower sensuous, non-reflective, and perceptual mental activities is a valid and valuable one, and one which is too often lost sight of. And we hold, further, that our author is right, in the main if not entirely, in denying to brutes the higher powers of conceptual thought. Again, we agree with Dr. Mivart in regarding the progress and development of sense-perception and abstract thought as more or less divergent. Where we part company with him is in the assumption, for such it appears to us, that these divergent lines of development cannot have a common origin. In all that he has written on the subject we fail to find any adequate justification for this dogmatic assertion so often and so confidently reiterated. The distinction between mere sense-perception and reflective thought is frequently drawn with admirable lucidity and clearness; but the impossibility of their having a common psychical root is merely asserted with a few rhetorical flourishes. We venture to question the assertion. Dr. Mivart is not, be it noted, content to assume the modest but perfectly legitimate scientific position that no one has yet succeeded in showing the early stages of the divergence, the tentative beginnings of the reflective process, the gradual focussing of the mental eye upon the processes of consciousness. He does not take his stand on a "not proven," but on a somewhat dogmatic "impossible"—not merely an "impossible" to this or that or the other factor in evolution from the nature of the factor, but broadly and generally an Impossible that is as worthy of a big initial letter as the Unknowable itself!

We cannot take leave of Dr. Mivart's volumes without again calling attention to the fact that they are full of matter interesting to the student of evolution. His scientific conclusions are not altogether those to which

we have ourselves been led, though there are not a few matters on which we have the pleasure of agreeing with him; his psychological and philosophical views are not in all respects those which we have reached, though here again we are on many not unimportant questions on his side; but we believe him to be an honest and fearless inquirer after that Truth which stands on the title-page of a work to which, perhaps, for some of our readers, these volumes of essays may form a suitable introduction.
C. LL. M.

PHYSICAL OPTICS.

A Treatise on Physical Optics. By A. B. Basset, M.A., F.R.S. (Cambridge: Deighton, Bell, and Co., 1892.)

A NEW treatise on the higher branches of physical optics must be welcome to all who are interested in the subject. Mr. Basset explains in the preface the scope and aim of his book, and it is needless to say that he performs the task he has set himself with ability and success. If, nevertheless, we close the book with a feeling of disappointment, it is because we could have wished that the author had been more ambitious, and attempted to give us a little more than a compilation of the standard papers on the subject. There is one sentence in the preface which, though it evidently does not express what the author meant to say, yet may serve as a peg whereon to hang the only criticism which can fairly be raised against Mr. Basset's treatment of his subject. "I have a profound distrust," says the author, "of vague and obscure arguments based upon general reasoning instead of upon rigorous mathematical analysis." Now, if we are to have vague and obscure arguments, it does not seem to matter much whether they are founded upon general reasoning or upon mathematical analysis, however rigorous that may be. In a subject which is in a state of growth, it may be possible to hide, but it is impossible to avoid, all obscurity and vagueness; and original work ever consists in the attempt to overcome such obscurities. By purposely excluding everything that is vague from a physical treatise, we destroy all possibility of making the work useful in stimulating further research. There are two ways of dealing with difficulties: we may try to overcome them, or we may run away from them. Mr. Basset chooses the latter course, and though some of us might have wished him to be a little more venturesome, we gratefully accept what he has given us, and the above remarks only apply to certain parts of the book. After an introductory chapter, Mr. Basset treats of the interference of light. He follows the time-honoured custom of taking Fresnel's mirrors and the biprism as the simplest case of interference. The effects which are observed are seriously modified, however, by so-called diffraction effects, and we might perhaps have expected a book of this kind to have entered a little more fully into the subject. That the author avoids all reference to experimental details is a distinct advantage, and renders his book more lucid and valuable for reference. It is much to be wished the author's plan could be more generally followed, and that all lengthy discussions of instrumental details could be kept out of theoretical treatises, and relegated to separate books.

The diffraction of light is fully discussed in chapters iv., v., and xiii. Mr. Basset has followed safe guides in the treatment of his subject, and it is perhaps this part of the book which will be specially valuable to the teacher and student.

It is well that the phenomena of double refraction should be first approached without more allusion to the difficult subject of the constitution of the ether than is absolutely necessary, and this is perhaps most easily done by following, as Mr. Basset does, the historical method, and starting from Fresnel's deductions.

The colours of crystalline plates are, of course, treated in an important chapter, and it is worthy of note that Mr. Basset does not introduce the somewhat misleading distinction between the effects produced by parallel and by convergent or divergent beams. In the usual polariscopes a number of parallel beams pass through the crystalline plate in different directions. If the optical arrangement between the plate and the eye is such that these various beams enter the eye, we get the phenomena which are often called interference effects in divergent light, while if those beams only which make a small angle with each other are allowed to pass the pupil, we get the uniform tint described as the effect of parallel light. Both kinds of effects might also be produced if instead of parallel beams we had a number of pencils diverging from points in a plane close to the crystalline plate. In either case the eye is supposed to focus for an infinite distance, and the different appearance is only one of degree, depending on the extent of the angle between the different rays passing through the crystal and into the eye.

Mr. Basset enters fully into the consequences of the various hypotheses which have been made as regards the differences of density or elasticity of the ether in different media. The investigations referred to by him are, of course, of the utmost importance, but it should have been pointed out that as regards application to optics they are wanting in reality. We know enough now to be able to say that the medium does not behave like an elastic body, and in some form or other the electro-magnetic theory must be considered as established. It seems idle, therefore, to discuss whether the hypothesis of Green or of Neumann is most contradicted by experiment. It would have been perhaps worth while to bring out more clearly the fact that *no* elastic theory of the ether has yet been found satisfactory, and that if the electro-magnetic theory had not come to help us we should be in a very serious difficulty.

It is true, of course, that we are at present unable, and probably always shall remain unable, to discard the elastic theories, because the study of transverse vibrations can only be satisfactorily carried out with the help of examples in which we understand to some extent the mechanism by which the vibrations are propagated. But unless a writer chooses to follow a purely historical treatment, it would seem to be more satisfactory to separate completely the mathematical study of vibrations from the subject of optics. Treated purely as elastic vibrations we may usefully discuss what would happen at the boundary between two media having different elasticities or densities;

and such a discussion, though independent of optics, would be certain to have important applications in it, because its results would often still apply when translated into language of the electro-magnetic theory. The mathematical investigation of vibrations might be made more clear and definite when it is freed from the necessity of adapting itself to experimental verification.

Chapter xviii. is a useful one, dealing with "theories based on the mutual reaction between ether and matter," but we might have wished for a more satisfactory introduction to the electro-magnetic theory that is given in the last two chapters. The way in which the subject is approached may illustrate some of the remarks made in the beginning of this review. There is no doubt a very serious difficulty in explaining the fundamental notions underlying the theory, and Mr. Basset, instead of making an attempt to help the student over the difficulty, suddenly plunges into a series of equations, referring us to Maxwell's book for an explanation even of his symbols.

We have perhaps given an inadequate idea of the contents of Mr. Basset's book, which no doubt lends itself to criticism from the physicist's point of view, but which nevertheless fills a gap and possesses merits which will render it of great value to the student of optics.

ARTHUR SCHUSTER.

THE APODIDÆ.

The Apodidæ: a Morphological Study. By H. M. Bernard, M.A. Cantab. (London: Macmillan, 1892.)

THE title of this little book is misleading. It is not a treatise on the Apodidæ, but a statement of the author's speculations on the relations of the Phyllopodous Crustacea and Branchiate Arachnida to the Chætopod Worms. The new observations recorded are few, and the most important, that as to the presumed hermaphroditism of *Apus cancriformis*, quite insufficiently set forth, and, so far as can be judged from the author's meagre statement, erroneous.

Mr. Bernard appears to be completely misinformed as to current views on the relationships of Apus to other Crustacea, and of that group, through it, to the parapodiate worms. Apparently he addresses himself to a lay audience, and poses, to start with, as the discoverer of a new and unsuspected agreement between the lower Crustacea and the Chætopoda. This may serve to excite the interest of uninstructed readers, but the zoologist knows that such pretensions are due either to defective acquaintance with the subject or to a want of candour on Mr. Bernard's part. The arguments by which Mr. Bernard endeavours to support his thesis are, many of them, those which have been effectively used by his predecessors in the same cause; others are new and remarkable only for their arbitrary character and the evidence which they give of the author's boldness in writing a book on a morphological problem. Mr. Bernard draws attention to the absence of developed articulations in the limbs of Apus as giving them a resemblance to the parapodia of Chætopoda. He states that this absence "has already been pointed out by Lankester and others, but its true significance does not seem to have been noticed." This is an incorrect allusion to my essays on the appendages and nervous system of Apus (*Q. J. Micr. Sci.*, 1881), and on Limulus an Arachnid (*ibid.*), which is the more to be regretted since they appear to have furnished Mr. Bernard with such of his theories as well as his facts as will bear examination. At p. 368, *loc. cit.*, my statement runs—

"I have long been of the opinion which Prof. Claus appears to hold, that the appendages of the Arthropoda are homologous (or, to use a more distinctive term, 'homogenous') with the appendages of the Chætopoda, and on this account I consider it a proper step in classification to associate the Chætopoda with the Arthropoda and Rotifera in one large phylum—the Appendiculata."

Yet Mr. Bernard comes forward to tell us that he now for the first time draws attention to the true significance of the absence of articulations in the limbs of Apus, although (as he admits) this condition was especially noted and very carefully described eleven years ago by me in the same essay in which the above paragraph as to the relationship of Arthropoda and Chætopoda occurs. This is a sample of Mr. Bernard's method of claiming novelty for what he has to say when dealing with old materials. Frequently he asserts in strong language novel propositions which are purely speculative and of the truth of which no evidence is adduced. There is in no part of this little book any evidence that the author has made use of living or of well-preserved material, or has had any special opportunities of studying the genera and species of Apodidæ; nor does it appear that he has any experience as a zoologist which might give some weight to his fanciful conceptions. On the contrary, these crude speculations and dogmatic assertions are his first original contributions to zoological literature. I regret to be obliged to say that in my opinion (which I am called upon to express candidly in these pages) "The Apodidæ" is not a book which can be recommended either as a repository of fact or as a model of the method in which a morphological problem should be attacked.

E. RAY LANKESTER.

OUR BOOK SHELF.

Anatomy, Physiology, Morphology, and Development of the Blow-fly (Calliphora erythrocephala). Part III. By B. Thompson Lowne, F.R.C.S., F.L.S. (London: R. H. Porter, 1892.)

WE have before us another section of Mr. Lowne's work, which has grown upon the author's hands, and will form two volumes instead of the one originally intended. Part iii. is occupied with the internal anatomy of the imago, embryonic development, histology, and the development of the imago. On each of these heads a great amount of information is supplied, and the author's statements are illustrated by many figures. As to the puzzling question of the way in which the alimentary canal of the blow-fly is developed, Mr. Lowne holds an opinion which is probably shared with no second person. What Voeltzkow and Graber take to be the proctodæum, and what Korschelt and Heider believe to be the amniotic cavity, Mr. Lowne calls archenteron. He is content, as he tells us in his preface, to await the verdict of posterity on such conclusions as this. We are content to wait too. The subject is too difficult for full consideration in this place, and it would be unfair to express a strong opinion without ample discussion of the evidence. It is not unfair, we think, to characterize many of Mr. Lowne's

morphological speculations as simple mistakes. To compare an insect-embryo and its membranes with a Lamellibranch or an Ascidian in the extempore manner assumed so lightly by Mr. Lowne (p. 244) is not creditable. He tells us that he has no facts to guide him except the similarity of the form and disposition of the parts. Any reader who is not able to judge for himself should be very much on his guard when our author mentions Vertebrates or Ascidians, or indeed any other animals outside the class of Insects.

It is painful to speak with any disrespect of an author so laborious and so independent as Mr. Lowne. But these good qualities do not suffice to make a really good book. Advice will probably be thrown away, but we will offer one hint in the most friendly way. If Mr. Lowne before going to press would get his sheets revised by any cautious and well-informed zoologist, he would be saved from making statements which seriously impair his work.

L. C. M.

A Mendip Valley: its Inhabitants and Surroundings. By Theodore Compton. With Original Illustrations by Edward Theodore Compton. (London: Edward Stanford, 1891.)

THIS is an enlarged and revised edition of the well-known "Winscombe Sketches," and will be cordially welcomed by readers who can appreciate the presentation of natural facts in a poetic spirit. The author has spent the greater part of "thirty-three years of rural life" in the valley about which he writes, and every aspect of it he knows and loves. He tells much that is interesting, not only about the valley itself, but about the people who inhabit it, and about its archæological remains, its wild beasts, past and present, its birds, fish, reptiles, butterflies, and flowers. The style is simple and clear, and a certain charm is added to the writer's descriptions by the quaint reflections with which many of them are associated. An excellent chapter on the geological history of the Mendips is contributed by Prof. Lloyd Morgan. The illustrations are daintily conceived and executed, and harmonize well with the general tone of the text.

Key to Elementary Dynamics. By S. L. Loney, M.A. (Cambridge University Press, 1892.)

THOSE who are using the author's Elementary Treatise, whether they be teachers or students, will find this key very useful. The solutions to the examples are here worked out in full, so that even one who is going through the subject by himself will learn much in the nature of attacking problems by direct methods. The author's treatise is now so widely used that this key will come as a great boon to many.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Lightning Spectrum.

DURING the brilliant display of lightning on the evening of June 28, I took the opportunity of making some observations of the spectrum. The way in which the spectrum varied was very remarkable, some of the flashes giving apparently perfectly continuous spectra, while others gave a spectrum of bright lines, as already recorded by Kundt and others. The continuous spectrum appeared to be associated with the flashes of longest duration, which were accompanied by very little thunder, and the bright line spectrum with the more instantaneous flashes. Using a Liveing direct-vision spectroscope with a very accurate scale, I succeeded in measuring the positions of six lines in the green, all of which no doubt have been *observed* before, but in two cases at least the *positions* have not been previously measured. The wave-lengths of the lines observed were as follows—those determined by Vogel, Schuster, and Colonel John Herschel, being added for comparison:—

	Schuster.	Vogel.	Herschel.	Remarks.
(1) 5002	—	5002	5009	Brightest line
(2) 5168	5160	—	—	Rather dim
—	5182	5184	—	—
(3) 5350	5334	5341	—	Fairly bright
(4) 5430	—	—	—	Rather dim
(5) 5515	—	—	—	Fairly bright
—	5592	—	—	—
(6) 5675	—	—	—	Fairly bright

Other lines were seen both in the red and blue, but time did not permit any accurate determinations of their positions.

The lines (1) and (6) are undoubtedly the two brightest double lines of the air spectrum which occur in this region, but in the case of the other lines the coincidences are not so definite. The proximity of the line 5168 to the brightest carbon fluting (λ 5165) would suggest that it has its origin in the carbonic acid gas, which is always present in the atmosphere. The remaining lines do not appear to coincide with air lines, and their origins for the present are undetermined.

A. FOWLER.

Royal College of Science, South Kensington.

On the Line Spectra of the Elements.

PROF. RUNGE has not improved the position he has taken up by the new instance of a motion which he brings forward in last week's NATURE. The instance he gave in his preceding letter is a motion which, as I pointed out, could not take place within molecules. The motion he now gives is one which cannot even exist anywhere in nature. It would require a supply of power (energy per unit of time) increasing *ad infinitum*. The first instance he gave belongs to inapplicable kinematics, his new one to impossible dynamics. Neither has anything to do with the subject of my memoir.

He quotes the enunciation of a theorem from chapter iv. of my paper, but does not quote the sentence introducing that theorem, which would have made it plain that the motions spoken of in it are *motions which can take place within molecules and which can produce an undulation in the ether*, not the motions of a mere mathematical exercise irrespective of whether they are real or imaginary. The introductory sentence (p. 588) is in the following words:—"The motions of the electrons, the electric charges in the molecules, which are what excite the ethereal undulation, may be motions that are not confined to one plane. Accordingly *to study them* we must investigate what theorem corresponds to Fourier's theorem when the motion takes place along a line of double curvature." And then follows the demonstration and the enunciation quoted by Prof. Runge. In the foregoing words, in the introductory paragraphs of chapter iv. of my memoir, and in other passages scattered up and down through that chapter, I made it abundantly clear, as I thought, that I was dealing throughout *with a real physical problem of nature*, not engaging in mere mathematical exercises that travel into the infinite and impossible. I now see that I ought to have made more explicit statements upon this point for readers who would judge of each sentence apart from its context.

In order that a motion, $x=f(t)$, may be susceptible of treatment by Fourier's theorem, the following are conditions that must be fulfilled:—

1°. The motion must be recurrent, or capable of being approximated to by recurrent motions.

2°. The quantity represented by x must not become infinite.

3°. The quantity represented by t must not retreat.

I have been familiar with these limitations since I was a student, more than forty years ago. *They are known to all students.* I therefore thought it superfluous, and still think it ought to have been superfluous, to state them in my memoir. I thought it also irrelevant, *since none of the limitations could occur in the motions I was investigating*; and I wished to shorten my memoir by excluding all irrelevant matter. Prof. Runge, however, objects that I have not treated of violations of the first

two of these conditions. He has not yet said that I ought also to have discussed the impossible dynamics in which the third condition would be violated.

This, however, was not his original position. He began (see his original article, NATURE, April 28, p. 607) by supposing that a motion from which light emanated cannot, if non-periodic, be investigated by Fourier's theorem; and he stated that in consequence of this he could not understand the decomposition of the motion of an electron within a molecule into a series of superposed elliptic motions. In NATURE for May 12, p. 29, and for June 9, p. 126, I demonstrated in two different ways that his supposition was a mistake. The other objection made in his original article, viz. that "a plausible suggestion about the movement of the molecules ought to explain more," is also a mistake. These are the two condemnations passed on my paper in his original article. Both these have been met. And the issues he has since raised are, I again submit, not *legitimate* criticism of a physical inquiry. To make them legitimate he would need to produce an instance of a motion *of the kind with which my paper deals* (*i.e.* a motion that can produce a spectrum) and which at the same time is not amenable to the method of analysis given in chapter iv. of my memoir. This he cannot do, for there are no such motions. In fact, the analysis effected by the spectroscope is *identical* with a part of that made by Fourier's theorem when applied in the way that I there point out. The spectroscope gives the periodic times in the different partials, the sum of the squares of their principal axes, and in some cases their forms; but it does not give the phases of the motions in them or the planes in which they lie. Prof. Runge almost admits that his criticisms do not succeed in impugning the value of my memoir as a contribution towards our knowledge of nature, for in his last letter he says, "I do not say, therefore, that Prof. Stoney's views on the cause of the line-spectra are wrong." This is very different from what he said in April.

G. JOHNSTONE STONEY.

9, Palmerston Park, Dublin,
July 17.

"The Grammar of Science."

MAY I, through your columns, point out to Prof. Pearson what seems to me a serious "antinomy," to use his own phrase, in his "Grammar of Science." The foundation of the whole book is the proposition that since we cannot directly apprehend anything but sense-impressions, therefore the things we commonly speak of as objective, or external to ourselves, and their variations, are nothing but groups of sense-impressions and sequences of such groups. But Prof. Pearson admits the existence of other consciousnesses than his own, not only by implication in addressing his book to them, but explicitly in many passages. He says (p. 59): "Another man's consciousness, however, can never, it is said, be directly perceived by sense-impression; I can only *infer* its existence from the apparent similarity of our nervous systems, from observing the same hesitation in his case as in my own between sense-impression and exertion, and from the similarity between his activities and my own."

With respect to the argument from the "similarity of our nervous systems," I may point out, *en passant*, that however many other people's nervous systems Prof. Pearson may have dissected, he has certainly never dissected his own, and that therefore this argument, which is several times repeated in the book, is worthless; all Prof. Pearson has to go upon is the external similarity of other people's bodies and activities to his own. But he maintains that our bodies and their activities are nothing but groups and sequences of sense-impressions. Consequently, if other consciousnesses are similar to his own, some of his groups of sense-impressions possess private consciousnesses, which themselves receive sense-impressions, among which, for example, are to be found Prof. Pearson himself! Thus Prof. Pearson's consciousness contains a number of parts, each of which contains, amongst other things, Prof. Pearson and his consciousness! Of course it would be impossible thus to refute a consistent idealist, who maintained that not only external things but all other consciousnesses were unreal and existed only in his imagination; but to recognize the reality of other consciousnesses is to recognize the reality of the means by which we become aware of them, which, as Prof. Pearson explicitly states, is the external aspect of men's bodies.

It is not difficult to find the way out of this difficulty. It is that, though we do not *know*, *i.e.* directly apprehend, anything about the external world but sense-impressions, yet in order to explain those impressions we frame the hypotheses of external, objective reality, and of the "ejective" reality of other consciousnesses, and since these hypotheses are successful in explaining most of our sense-impressions, we have come to believe that they are true. Indeed, I cannot seriously doubt that Prof. Pearson himself believes in them as much as anyone else. Only, if he were to acknowledge it explicitly, he would have to rewrite almost every page of "The Grammar of Science."

EDWARD T. DIXON.

12 Barkston Mansions, South Kensington,
July 14.

PROF. KARL PEARSON'S "Grammar" merits more justice than it has received from "C. G. K." It is a remarkable book which I have read with much interest. He tells us (p. 15) that "the unity of science consists alone in its method, not in its material," and therefore the method employed in this work on science acquires a special interest.

There are two points in respect to which his method seems to me to call for a few remarks—remarks which cannot be unwelcome, since his motto is "La critique est la vie de la science."

The first point concerns his own position and that of certain persons he freely criticizes. The Professor has scant patience with metaphysics, and says not a few hard things of those tiresome people the metaphysicians; and yet his own book is really a metaphysical treatise and he turns out himself to be an unconscious metaphysician *malgré lui*. This fact can hardly surprise anyone who has mastered what is really the scientific A B C; but in the present instance it is peculiarly amusing. For he, with great *naïveté*, ridicules Prof. Tait for being in the very same case. He is styled (p. 296) "the unconscious metaphysician, who groups sense-impressions and supposes them to flow as properties from something beyond the sphere of perception"; and we are further told that "the unconscious metaphysics of Prof. Tait occur on nearly every page of his treatment of the fundamental concepts of physical science."

The second point which seems to require notice is the way in which his method plays "fast and loose" both with the system he upholds and the system he most opposes.

He is an idealist of a kind. Again and again we are told that scientific laws are but descriptions of our feelings in conceptual shorthand. He speaks (p. 129) of "the whole of *ordered* nature" being "seen as the product of one mind—the only mind with which we are acquainted," and he tells us plainly (p. 130) that "the mind is absolutely confined within its nerve-exchange; beyond the walls of sense-impression it can logically infer nothing." It would be easy to multiply such quotations.

Now, of course the idealist can logically make use of ordinary language in describing co-existences and successions between his feelings. The Professor's distinctions (p. 114) between "physical and metaphysical supersensuousness" have been duly noted, as also his disclaimer (p. 57) of giving any real explanation of the physical side of thought. Nevertheless, none of these considerations appear to me to justify his dogmatic mode of speaking of things of which the senses can take no cognizance.

If he knows nothing but his own feelings, he cannot reasonably speak of their mode of formation, or of the manner in which one group of feelings acts upon another. Yet, referring to a sensory nerve, he writes (p. 51): "The *manner* [the italics are mine] in which this nerve conveys its message is, without doubt, physical," and (p. 81) "Beyond the brain terminals of the sensory nerves we cannot get." Stars are for him but "groups of feelings," and yet he writes about them as follows: "Among the myriad planetary systems we see on a clear night, there surely *must be* myriad planets which have reached our own stage of development, and teem, or have teemed, with human life" (p. 179).

Speaking of waves (p. 305) he tells us, "The wave *forms* for us a group of sense-impressions." But the wave is, for him, *itself* a group of self-impressions and so is a particle of protoplasm. Nevertheless he speaks (p. 413) of the probability that long stages of development preceded its existence, and "of the millions of years, with complex and varying conditions of temperature," needed in order "to pass from the chemical *substance* of life to that complex *structure* which may have been the first

stage of organic being." He also declares (p. 425) the belief that "the evolution of organic nature is at the basis of human history is the *unswerving belief* of the present writer."

My present object is not to object to any of these statements, but simply to call attention to the complete accord which exists between the Professor's language and that of realism, or any of the materialists whose sayings he sometimes deprecates, and to note the practical outcome of such teaching as that of his metaphysical "Grammar."

Its Idealism is an Idealism of parade, to be brought out occasionally, above all to confound some rash or inexperienced advocate of Intellectualism and Common-sense. But ordinarily and habitually it most certainly is, as "C. G. K." affirms (NATURE, July 7, p. 222), "distinctly materialistic." This teaching is an excellent example of that "intellectual thimble-rigging"—I use the illustration as an apt one, but in no invidious sense—to which I have elsewhere (see *On Truth*, p. 135) called attention in greater detail.

In conclusion I would ask how Prof. Pearson's metaphysical system can be necessary or even useful for the progress of science?

What does it matter for science, provided we are all agreed about those things whereof the senses can take cognizance, whether or not we are convinced that something extended exists objectively? The Professor affirms (p. 215) that the man of science "refuses to project his conceptions, atom and ether, into the real world of perceptions until he has perceived them there." We are then so far agreed. We both welcome ether units, prime atoms, chemical atoms, molecules, molecular motion, ether rings, ether squirts, &c., as admirably useful working hypotheses, but not as things to be yet regarded as objective realities. If I am right, the utility for science of much of the Grammar is not easily to be recognized. But it has a very distinct metaphysical utility for the opponents of the system Prof. Pearson favours, and will no doubt meet with grateful recognition at the hands of some of them.

ST. GEORGE MIVART.

Hurstcote, July 16.

A "Viper" Bite.

As cases of poisoning from the bite of venomous reptiles are happily rare in this country, it may prove interesting to some of your readers if I relate my experience on this matter.

About a month ago I caught two snakes at Bickleigh, near Plymouth, and whilst examining one it "bit" or rather struck me on the lower part of the right thumb. I immediately sucked the puncture (it could not be called a wound) which bled a little, and tried to make light of the matter. A livid patch soon formed round the point, and the hand and arm commenced to swell. In a quarter of an hour I was unable to hold anything, and almost in a fainting condition. The first symptom (apart from the swelling) was a peculiar taste and a sensation of swelling in the teeth, then the tongue commenced to swell and became so large that I could hardly move it, my eyes seemed ready to start from their sockets.

In half an hour a terrible vomiting commenced, preceded by excruciating pains in the stomach and heart, and continued with the pains altogether for nine hours, every drop of liquid being ejected almost as soon as swallowed; there was also violent purging and complete suppression of urine.

There was practically no pain in the arm; altogether the painful symptoms lasted for about nine hours.

I did not lose consciousness at any time. The arm continued to swell for two days, and then it was nearly as large as my leg. After this the swelling subsided, but the arm did not return to its normal size until twelve days after the accident. After the swelling had gone I suffered very much from rheumatical pains, and in fact do so now, and the digestion was also very much impaired. The viper is a male, a little more than two feet long, and about one inch in diameter at the largest part. Colour, a dull yellowish brown on the upper side, with a zigzag black line running down the whole length. On the under side it is nearly black except at head, where it is pale yellow. I have kept the reptile now for nearly five weeks, and although well supplied with small frogs, &c., it has not eaten anything, and seems as lively as ever.

Cases of this kind, where the sufferer is able to record the symptoms, being rather unusual, is my excuse for occupying the space of NATURE.

Plymouth. W. A. RUDGE.

THE EDINBURGH MEETING OF THE BRITISH ASSOCIATION.

THE Association has already met three times in Edinburgh, in 1834, in 1850, and in 1871. With the success of the 1871 meeting fresh in the memories of many citizens, the Town Council and other public bodies have entered cordially into the local arrangements for the meeting. The local committee formed some twelve months ago and its sub-committees have been actively at work, and everything is now practically ready for the reception of the Association.

The number of members of the Association who have indicated their intention of being present, and of new members who have already joined, are such as to show that the meeting will be an exceptionally large one. More than fifty distinguished foreigners have accepted the invitation of the local committee to attend the meeting.

Reception and Section Rooms.—The reception rooms are in Parliament-square, adjacent to St. Giles' Cathedral and the City Chambers. The Parliament Hall, the various court rooms, the rooms of the Society of Advocates, and the new library and hall of the Solicitors before the Supreme Courts have been placed at the disposal of the committee, and have been so appropriated as to constitute an ideal suite of reception rooms, including secretaries' and treasurer's offices, post, telegraph, and telephone office, ticket office, enquiry office, reading room, writing room, ladies' boudoir, smoke room, and refreshment buffet. Many of the rooms lend themselves to decoration, and the arrangements are as excellent in taste as in convenience. The Section Rooms are all in the University buildings; Sections A, E, F and G in the old buildings, and B, C, D and H in the new buildings. These buildings are about two minutes' walk from one another, and about four from the reception rooms. The section rooms are all well adapted for the purposes of the meetings, and in connection with each there is ample accommodation for committee meetings, while provision has been made for the occasional subdivision of some of the sections. In the new University buildings a room has been set apart for a temporary museum, in which objects of interest, which are brought under the notice of any of the sections, may be afterwards placed so as to be more easily inspected than is possible during the meeting of the section. It is expected that this will prove a valued addition to the convenience of the meeting.

While light refreshments may be had at the buffet in the reception rooms, the principal luncheon room will be found in the Students' Union Club, situated between the new and old University buildings. In the club there will also be a ladies' room, smoking-room, billiard-room, &c.

Lectures and Entertainments.—The programme for the evenings will follow the usual lines:—On Wednesday Sir Archibald Geikie will assume the presidency and deliver an address; on Thursday, the Lord Provost, Magistrates and Town Council invite members and Associates to a *conversazione* in the Museum of Science and Art; the Lord Provost will receive and welcome guests to the City. On Friday, Prof. Milnes Marshall will lecture on "Pedigrees"; on Saturday, Prof. Vernon Boys will lecture to artizans on "The Photography of Flying Bullets"; on Monday, Prof. Ewing will lecture on "Magnetic Induction," and on Tuesday there will be a *conversazione* in the Music Hall on the invitation of the local committee.

Military bands will play in the Princes Street Gardens every afternoon during the meeting, and there will be organ recitals in the "Reid" music class-room. Afternoon entertainments will be given by the Royal Scottish Geographical Society, the Rector and Masters of the Edinburgh Academy, and by others.

Arrangements have also been made to form parties to visit Edinburgh Castle, Holyrood Palace, and Arthur's Seat; these visits will be in the afternoon.

Excursions.—The committee have prepared a long list of excursions. Among those for Saturday afternoon are geological excursions to North Berwick and Tantallon, and to the Pentland Hills; a botanical excursion to Gullane; a dredging excursion on the Firth of Forth; and excursions to such places of interest as the Land of Scott, the Fairfield Shipbuilding Works and Glasgow, the Pumpherston Oil Works, Dundee and the Firth of Tay, Stirling, Rosslyn, Dalmeny and the Forth Bridge, Newbattle Abbey, and Dalkeith Palace.

On Thursday, occasion is taken to visit places of interest further afield. St. Andrews, Dunkeld, Scone and Murthly (arboricultural), Croy (archæological), Dobbs Linn Moffat (geological), Moorfoot Waterworks, Hamilton Palace, Drumlanrig, Yarrow, Crieff, the Trossachs, Loch Lomond, the Firth of Clyde, are all brought within the limit of a one-day excursion.

Many of the more important manufacturing and other works in the city and neighbourhood are to be open to members, who will thus have ample opportunity of becoming acquainted with the trade of the district. Visits to the paper works at Penicuik or Currie, to the printing-ink works at Granton, and to the gunpowder mills at Roslin, will form pleasant short afternoon excursions. The printing offices of Edinburgh are of great interest, and many of them have made arrangements for the reception of visitors. Breweries, distilleries, biscuit factories, and hydraulic engineering works have all their special developments here, and are well worthy of visits.

Hospitality and Lodgings.—Perhaps the greatest difficulty that the local committee has had to face has been the date fixed for the visit of the Association. August is *the* holiday month in Edinburgh, and under ordinary circumstances the residential parts of the town are during that month entirely in the hands of the police. For many of the citizens, indeed, holidays are possible only in August. It has therefore been matter of congratulation to the committee dealing with this part of the work to find that many people intend to remain in town during the meeting of the Association and that they have been informed of a large number of offers of hospitality having been sent to visitors.

The hotel accommodation in Edinburgh is considerable, but the strain upon it in August is great. The local committee have secured for members of the Association a considerable number of rooms in hotels, and these are being rapidly allotted on application. Visitors who intend to live in hotels during the meeting will do well to make their arrangements early.

With regard to lodgings, probably no town is so well off as Edinburgh, and fortunately during August many of the best rooms are vacant. A register of lodgings has been opened at the local offices, and the secretaries are prepared to give assistance to visitors desiring to secure apartments. A provisional list of hotels and lodgings has been prepared and may be had on application. The principal clubs have offered to admit visiting members of the Association as honorary members during the meeting, subject to such conditions as are required by the constitution of the club.

Publications.—The programme of local arrangements will contain a hotel map of Edinburgh, a large scale map of central Edinburgh, including all the buildings used in connection with the meeting of the Association, and a general map of Edinburgh and Leith, on which all the works open to visitors are specially marked.

The "Excursions Handbook," published by the committee, gives details of the various transit arrangements and general sketches of the routes to be taken. It also indicates the nature of the interest attached to each excursion. The handbook will be illustrated by a special map of the South of Scotland and by section maps on a larger scale showing details of excursions.

F. GRANT OGILVIE.

THE ORIGIN OF LAND ANIMALS: A BIOLOGICAL RESEARCH.[1]

THIS remarkable and very unequal work, many-sided and heterogeneous, is worthy of careful consideration. It is not wanting in imagination, more or less disciplined, and it is loaded with information from the works of contemporary naturalists, now for the first time brought together in a single volume. One great merit it has of regarding plants and animals, not merely as forms of life, but as living forms: the machinery is exhibited to us in motion.

The title of the work scarcely conveys an adequate idea of its comprehensiveness; it might just as well have been styled "The Evolution of the Living World [for plants are not excluded from its universal purview], and the way it has been brought about."

The leading idea appears to be that a change from marine to terrestrial habitat has taken place much earlier in the history of the higher forms of life than is generally supposed, that the land from the early beginnings of geologic time has been peopled both with animals and plants, and has, more than the sea, been the great arena of progressive change. At the outset, the shore, where sea and land and air all meet and commingle, was the birthplace of life, and from it living forms have continually wandered in all directions—to the open ocean and the abyssal depths, to rivers, marshes, and dry land. From the Algæ, which are almost the only marine plants, the vegetable kingdom was derived. That this is characteristically terrestrial is due to the fact that vegetable protoplasm is less adaptive than animal. "Plants as land proprietors are the true conservatives;" hence, once on land always on land. The terrestrial character of plants offers a suggestive hint as to the place of development of the greater part of the animal kingdom: it also has been on land, but with more numerous offshoots to the sea. In terrestrial plants such as Myxomycetes—"the true Bathybius"—are the roots of the animal world; or if this claim be not admitted, and to Bacteria be assigned this place, a terrestrial origin remains unimpugned, since these organisms are predominantly inhabitants of the land.

The migration of marine animals may be direct, but more usually it is by successive stages, first through fresh water—"the great highway to land-life"—then to damp places, and finally to the dry land itself, which, however, at the time of migration may have been subjected to a damper and warmer climate than at present prevails. With change of medium progressive modification has been associated, for existence in the air makes three great demands on the organism, it must protect itself against being dried up, acquire new modes of respiration, and more substantial organs of support.

Many animals, ennobled by their response to these demands, have returned to the sea, and exercise dominion over it, undergoing, of course, fresh modifications, particularly of the respiratory organs; while others have retained possession of the terrestrial domain, adapting themselves to minor changes of habitat and climate. Thus far more groups of land-animals are derived from a terrestrial ancestry than we imagine, and the next-of-kin of orders now characteristically marine are less frequently than we suppose to be found in the sea, but must be sought for on the land. The whale and sea-turtle, land crabs and climbing fish, so far from being rare and exceptional cases, are instructive examples of great migratory movements and associated anatomical change.

The hypothesis not only supplies a needed stimulus, powerful enough to account for the evolution of the organic world, but at the same time it explains the futility of our search in marine strata for connecting links between lead-

1 "Die Entstehung der Landtiere: ein Biologischer Versuch." Von Dr. Heinrich Simroth, Privat-docent an der Universität, Leipzig. Pp. 492, with 254 Illustrations in the Text. (1891.)

ing types of life, since the most critical steps in evolution have been taken on the land, and terrestrial fossils are of the rarest occurrence.

In illustration we may select the author's treatment of the Arthropoda, which have their origin in some ancient Annelid, probably a marine Polychæte, and not an Oligochæte, since no Arthropod possesses the red blood which the Oligochæta have acquired as an adaptation to land life.

The absence of cilia and a thoroughgoing chitinization, which are the most striking peculiarity of the Arthropoda, are a direct adaptation to land life; the chitinous envelope furnishing on the one hand protection against desiccation, and on the other organs of support, whilst its extensive development necessarily involves the disappearance of cilia, and the development of fresh contrivances for respiration.

Another important character common to the Arthropoda is the transverse striation of the muscle fibre; but transverse striation is generally admitted to be correlated with excessive functional activity, from which, according to the author, it results. Encase an animal in chitin, and its movements will, from the mechanical conditions of the case, be "acrobatic,"—to move at all it must move strenuously, by this excessive exercise transverse striation will develope in all the voluntary muscles, and "by correlation" in those of the alimentary canal as well. So much is the author impressed by the cogency of this reasoning that he regards the striation of the musculature as a direct indication of the terrestrial origin of the animal possessing it, and ventures to apply this formula to Sagitta, the direct development of which he gives as an additional argument for its descent from some terrestrial species.

The parapodia of the Annelida naturally gave rise to the appendages of the Arthropods, and it was while these were still short, scarcely-jointed stumps that the Trilobites branched off in one direction, converting all their parapodia into legs, and the Scorpions and Merostomes, which discarded their abdominal appendages, in another. The Crustacea, retaining like the Trilobites all their appendages, branched off at about the same level, and their connection with the Arachnida is confirmed by Jaworowski's recent observation of the exopodital and endopodital splitting of the appendages in Tarentula. A confirmation of the terrestrial habitat

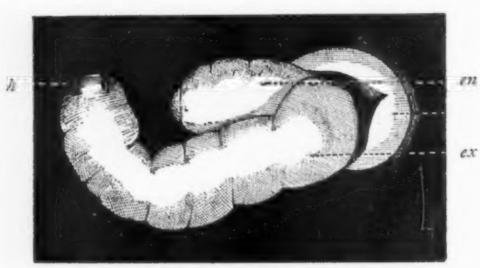

FIG. 1.—Pedipalp of an embryo of *Trochosa singoriensis*; *en*, endopodite; *ex*, exopodite; *h*, hairs (after Jaworowski).

of the primitive Crustacea is suggested by the fact that the most archaic existing forms are the Branchiopoda, which still live in fresh-water and salt marshes, can survive drying up, and indeed seem to require it for the production of sexual eggs. The remarkable diversity of the respiratory organs in the Crustacea is another important piece of evidence, since it points to their having been acquired as secondary adaptations.

Of Arachnoid forms, some entered the sea, probably the majority of the Merostomata and the Xiphosura, but Limulus still gives evidence of its original home, since it comes to the shore for begetting, and lays its eggs at the highest tide-mark.

No doubt the notion that the immediate ancestors of Limulus were land animals will excite scorn in prejudiced minds; but it is one that Balfour long ago suggested (the author does not seem to be aware of this), led to it probably by his recognition of the close relationship between Limulus and the Arachnoids on the one hand, and the Arachnoids and Insects on the other—the latter connection lately so much strengthened by Jaworowski's remarkable discovery of rudimentary antennæ in Tarentula. In

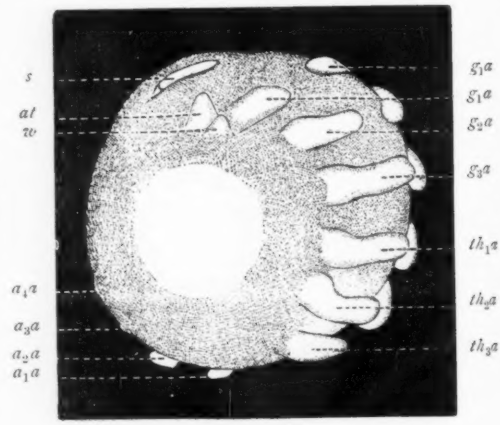

FIG. 2.

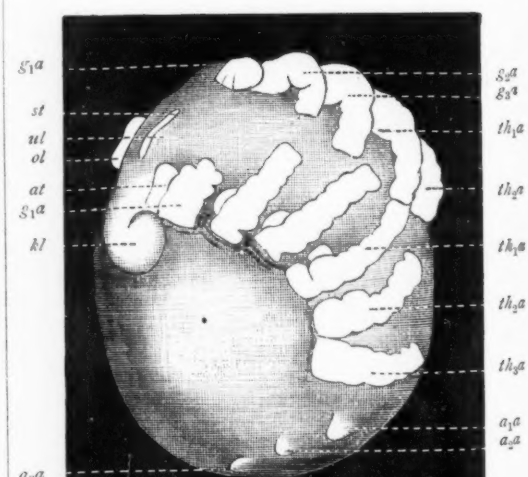

FIG. 3.

this direction may be looked for a reconciliation of the views of Lankester and Lang.

The mild surprise with which we learn that Trilobites and Crustacea were originally denizens of the land has scarcely given place to conviction before we encounter the chapter on fishes. We shall be prepared to find that these can claim terrestrial ancestry too. The earliest fossil vertebrates of which we know anything are the Placoderms; these were dwellers in the Old Red Sandstone lakes, and, as our author remarks, "from fresh water to the land is only a step." That the Placoderms were

well able to take this step is proved by the character of their pectoral limbs, which, unlike the fins of fish, are provided with a transverse joint in the middle—"an elbow joint"; and this, while clearly helpful in walking, would not be well fitted for swimming. No doubt the animal was also a swimmer; the dorsal fin shows so much, but it was also a walker, travelling over hard, uneven ground; indeed, to this habit is attributable the turning up of the tail-fin (!), which formed the third point of support. A

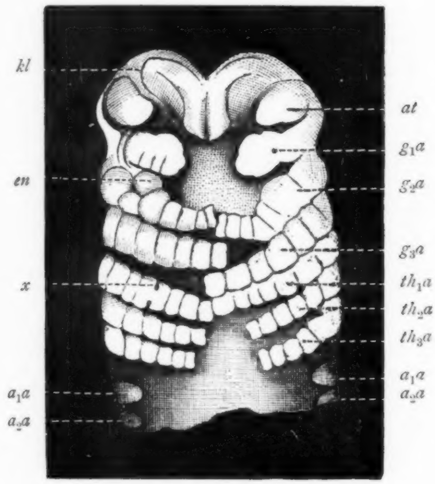

FIG. 4.

FIGS. 2 to 4.—Embryos of *Trochosa singoriensis*. Fig. 2 on the 13th day. Figs. 3 and 4 on the 15th day; Fig 4, the anterior end seen *en face*. *st*, stomatodæum; *kl*, frontal lobes; *ol*, upper and *ul*, lower lip; *at*, antennae; *w*, wall-like thickening of marginal groove; g_1a, chelicerae; g_2a, pedipalp; g_3a, 1st pair of limbs; th_1a–th_3a, thoracic appendages (2nd to 4th pairs of limbs); a_1a–a_4a, abdominal appendages; *x*, a deep constriction; *en*, rudiment of endopodite (after Jaworowski).

drawing, subscribed "original," representing Pterichthys "as it might have moved," is so full of unconscious humour that we are tempted to reproduce it. From such amphibious primitive vertebrates the fish branched off in one direction and descended to the sea—the swimming-bladder represents the original lung; in another direction proceeded the Stegocephala, the ancestors of reptiles, birds, and mammals. Primarily the Vertebrata are derived from Annelids, but the claim put forward for the Placo-

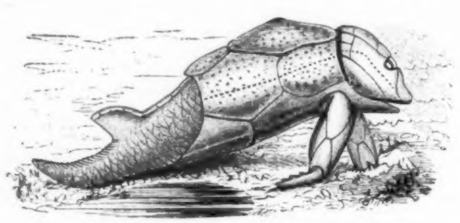

FIG. 5.—*Pterichthys*, as it might have moved. (Original.)

derms is more in harmony with Patten's view, connecting them with the Arachnoids; for the grave difficulties which beset this view, however, let Smith Woodward's trenchant criticisms be considered.

The main line of argument is followed into a number of collateral branches, all elaborately discussed. There is a powerful chapter on the strand fauna, in which are arrayed the great host of marine animals, including fishes, which temporarily leave the sea to breathe the air. This is regarded as a fact of profound significance, indicating a general tendency of the strand fauna to come on shore.

Recent investigations by Zacharias, Nusbaum, Chun, and others are made good use of in discussing the distribution of fresh-water fauna. Aërial transport, particularly by birds, is accepted as accounting for most of the facts. The survival of the transported forms is insured by the chitinous investment either of the animals themselves or more usually of their eggs. It is pointed out that most pelagic fresh-water species are provided with means of attachment: such are the spines of pelagic species of Daphnia, the abdominal processes of Bythotrephus, and the singular antennæ of Bosmina. Copepods which lay eggs which sink to the bottom are restricted in distribution; those which carry them about in egg-sacs are world-wide.

An attempt is made to prove that fresh water opposes some obstacle to the secretion of carbonate of lime; and though a comparison of the thickness of marine and fresh-water shells is far from bearing this out, yet some interesting results are elicited; as, for instance, the suggestion that the chitinous bristles of the young of *Paludina vivipara* are probably the last traces of originally calcareous spines.

In illustration of the various stages of land life, the Testacillidæ are cited as an interesting example of adaptation to a terricolous existence. *Daudebardia*, one of the family, begins life as a form precisely like a *Hyalina*, but with growth passes through the successive stages shewn in the figure (fig. 6) till it becomes the worm-like adult.

A good deal of space is naturally devoted to the subject of encystment, which is regarded as a protection against desiccation. In the course of this discussion an earlier origin is attributed to the Heliozoa than to the

FIG. 6.—*Daudebardia* in different stages of growth; on the right, youngest, on the left, oldest stages. The buccal mass is shaded. (Original.)

Radiolaria, since they do not possess the central capsule of the latter, which are consistently regarded as the marine descendants of an ancient fresh-water group related to Heliozoa. The suggestion is added that the withdrawal of plasma in the Radiolaria into the central capsule as a preliminary to spore formation is not really with a view to this event, but a reminiscence of encystment, which occurred in ancestral fresh-water forms. Bald suggestions such as this, and another which occurs in the work, to the effect that chlorophyll first acquired its fluorescence as the primæval sky cleared of clouds and permitted an extension of the solar light towards the violet end of the spectrum, should, from motives of prudence, have been omitted. A total *bouleversement* of accepted views on main lines of descent is sufficient for a great work without the added irritation of superfluous conjectures. Summer and winter eggs belong more or less to the question of encystment, and the author regards winter eggs as "an adaptation to small pools, and threatened destruction by drying up." This, like the statement that the chitinous shells of the eggs of pelagic Crustaceans were acquired as a protection against desiccation during their aërial flight, might have been expected from an ultra-Darwinian, but in an author who wishes to explain evolution by physical causes, and not by chance, it is less pardonable. "An adaptation to threatened drying up" is an expression which would please the metaphysicians, who have lately been contending that an effect may precede its cause.

The bibliography at the end of the work will be found most useful, especially to Englishmen, who will find in it a guide to a great deal of interesting German literature; but it is without form, and this to a great extent is true of

the work itself. There are citations to the number of 423, and more, not numbered, yet, although we have a long discussion on the relationship of Limulus to Scorpion, Lankester's work is not mentioned; with chapters on fresh-water faunas, no allusion to "The Origin of Fresh-water Faunas," by Sollas; W. Marshall, a German author, is set forward in the text as an authority on pelagic and coast faunas, and Moseley overlooked; titles are sometimes given without place or date of publication, a defect which becomes serious when periodical literature is referred to without mention of volume. The illustrations are numerous and excellent.

The author has produced a fresh and promising thought, but one cannot help regretting that he did not wait—like, say, Darwin—till it was full time for bringing forth. W. J. SOLLAS.

THE PHOTOGRAPHIC MAP OF THE HEAVENS.[1]

THE first number of the second volume of papers published under the auspices of the Permanent Committee charged with the execution of the photographic map of the sky has made its appearance at a sad moment in the history of the undertaking. For simultaneously with its appearance is announced the death of him who, more than any other man, has contributed to its success, and brought it within the range of practical science. Admiral Mouchez has known how to secure not only the active co-operation of many astronomers, but also how to make them zealous in the great work, the arrangement of the details of which has occupied the last years of his life. He has awakened enthusiasm for the success of his scheme, and smoothed many difficulties which might have hindered its progress, and probably few undertakings of equal magnitude and equal importance, breaking new ground in many directions, have been got under way with less friction and fewer disappointments. We may well hope that the same sauvity and diplomacy which has characterized the conduct of the late Director of the Paris Observatory will be found in the counsels of his successor, and that a work begun in so much hope will be carried to a successful issue.

The papers in the volume before us can be brought roughly under two heads, both, notwithstanding the lapse of time from the inception of the scheme, betokening an initial stage in the preparation. One of the topics under discussion has for its aim the selection of a method which shall secure on the photographic plates, destined ultimately to furnish a catalogue, the impression of stars of the eleventh magnitude with certainty and uniformity; the other, a means of deriving the co-ordinates of the star images so impressed with the greatest facility and sufficient accuracy.

To deal with the second of these proposals first, we may remind our readers that whatever method of measuring the positions of stars on a plate may be adopted, the resulting co-ordinates must be purely differential, and probably referred to the axes of the réseau impressed upon the plate as a latent image, and developed under the same conditions as the stars themselves. To pass to the determination of R.A. and declination, a great deal of information, entirely independent of photography, will have to be made available. The readiest means of effecting this last step in the reduction, as it appeared to a committee of experts appointed to consider this question, was to determine by meridian instruments the absolute co-ordinates of six stars on each plate. It is needless to comment upon the magnitude of the labour thus undertaken, or at least contemplated. This preliminary work would demand a catalogue of some sixty or seventy thousand stars, most of them below the ninth magnitude and not found in existing catalogues. In order to give to each determination the necessary accuracy, it is desirable that each star should be observed twice in both elements and at two observatories. When we remember the length of time that the re-observation of Argelander's zones has consumed, and is still incomplete, we can form some estimate of the time that must inevitably elapse before the results of the photographic catalogue can be made available for astronomical purposes.

In presence of these difficulties, and many more which occur to the practical astronomer, we must be very grateful to M. Loewy for elaborating a scheme which, if it be found practicable, will materially shorten the time necessary for the production of the catalogue. M. Loewy proposes to avail himself of the fact that the plates are taken in two series, in such a manner that each corner of a plate in one series will form the centre of four other plates in the second series. When, therefore, the astronomer has determined the rectilinear co-ordinates of the stars on *one* plate relatively to the central lines of the réseau, each of these stars will belong in common to the plate considered, and to one of the four plates of the second series, partially covering the first. M. Loewy's scheme consists in making the stars on the four plates thus connected available for the reduction of the first. And, on paper at least, it is not difficult to extend the scheme still further, and to make the plates contiguous to these four contribute to the reduction of the original plate by means of an extended triangulation. In this way a plate would not be considered as an isolated fact, but a considerable area, of 36, 64, 100 or more square degrees could be woven into a harmonious scheme of reduction. And such a plan possesses this very obvious advantage, that on even a lesser area, as of 36 square degrees, we may well expect to meet a sufficient number of bright stars whose places are already so well determined that the reduction of the plates could go on immediately without waiting for the observations of the stars on the meridian. And independently of this evident advantage, it seems highly probable that two of the elements of reduction, viz. the orientation of the plate, and the value of the scale, will be determined more accurately, if the stars which are used for the derivation of these corrections are separated by a considerable distance, that is greater than a single negative would permit.

M. Loewy considers the various sources of errors and their necessary correction with all the detail required to submit the plan to practical application, and this is precisely the test that is needed. This appears to be also the opinion of Dr. Gill, expressed in a very cautious approval of M. Loewy's scheme, and he further quotes a remark of Prof. Auwers, which contains a very salutary caution. That astronomer points out that the reduction of the catalogue plates will be most accurately effected from the position of *faint* stars, rather than from bright ones. In that case since our present most accurate catalogues do not give the positions of the fainter stars, those catalogues will still need to be supplemented by many meridian observations. Dr. Sande Bakhuyzen, however, expresses the opinion that the zones of the *Astronomische Gesellschaft* will, when completed, furnish the necessary data for all reductions, or, at most, require additional observations in some portions of the sky, which he is able to point out from a careful examination of the number of the stars contained in these zones.

The second topic which has received much consideration in this volume is, as before mentioned, the adoption of a method to secure the registration of stars of the eleventh magnitude. It will be remembered that the International Congress of 1891 proposed to place in front of the object glass of the telescope, screens of fine metallic gauze, identical in manufacture, and of such construction that the amount of light impeded should be equivalent to two magnitudes: the coefficient 2·512 being employed as

[1] "Bulletin du Comité International Permanent," tome ii., premier fascicule.

the ratio to express the relative brilliancy between two consecutive magnitudes. A committee was appointed to carry this plan into execution, but the report which this Committee has issued is unfavourable to the adoption of the method. The signatures of the Astronomer Royal, Prof. Pritchard, and the brothers Henry, are attached to this report; but M. Vogel, the remaining member of the Committee, has not found the reasons assigned by his colleagues sufficient to warrant the rejection of the scheme, and consequently his name does not appear. The President of the Permanent Committee thus sums up the case against the proposal. Light in traversing a metallic screen of bright threads and very narrow mesh, seems to experience, besides the ordinary effects of diffraction, certain modifications, whose cause is not yet explained, and which the Congress could not foresee when they framed the recommendation. This peculiar behaviour of the light demands further study, and renders the application of this means very difficult, if not useless, for the purpose for which it was proposed, since the discrepancies of the results obtained are greater than the error that an experienced astronomer would make in estimating stars of the eleventh magnitude.

The experiments on which this conclusion is founded are set out in considerable detail, and a careful study of these experiments ought to convince an unprejudiced critic that the committee was justified in advising the rejection of the screens as an adequate and efficient means of deciding upon stars of the eleventh magnitude. It should be stated that the gauze screens, identical in character, were furnished by Prof. Vogel, and though there is no mention of the experiments or processes which induced the Potsdam astronomers to select a screen of this particular obstructive power, it is to be presumed that in his photographic telescope they stopped the amount of light proposed by the Congress. It is not the least curious feature in the discussion (controversy would be far too strong a word to describe the courteous paragraphs in which the various astronomers set forth their reasons for dissent from the able physicist), that Prof. Vogel takes no part in it nor vouchsafes any information as to the principles by which he was guided in the selection, but leaves the onus of rejection entirely to his colleagues, who are thus placed at a disadvantage.

Prof. Pritchard, whose photometric researches permit him to speak with authority, has stated concisely the result of his experience. He found that on the ordinary astronomical telescope, achromatised presumably for D, the amount of light obstructed was equivalent to 2·4 mag., and on the photographic telescope, with a minimum focal length for G, the amount of light lost was not less than 2·8 mag. The Astronomer-Royal reports that the action of the screen on the Greenwich telescope is to stop 2·5 mag. This result was deduced by comparing the seventh and ninth magnitude stars of Argelander. Some further comparisons of the obstructed and unobstructed light of stars of the ninth and eleventh magnitude photometrically examined by Prof. Pritchard with the wedge photometer confirmed this result, and further proved that the scale of Pritchard and Argelander was in very satisfactory and close agreement. It will be necessary to return to this point. M. Henry at Paris offers results in close accordance with those of the two English astronomers just quoted. He finds that the screen proposed by M. Vogel as effective in his instrument stops between 2·5 and 2·7 mag. on the Paris telescope, and this effect is still further confirmed by some observations by M. Trépied, while M. Rayet at Bordeaux finds 2·7 mag. represents the effective action of the screen. Very different is the experience of M. Donner, of Helsingfors. His method of estimating the loss of light is different from that employed in the other cases, and is perhaps not without objection, but the result which he derives from his observations is that the light of a star in passing through the screen loses only 1·6 mag.

It is now necessary to describe very briefly the methods employed in the various observatories which have led to these discordant results, the more so as one eminent authority, Dr. Dunér, of Lund, who apparently holds a brief for Prof. Vogel, has taken exception to the results deduced. Leaving on one side the experiments conducted by MM. Henry and Trépied on artificial stars, and against which Dr. Dunér urges no objection further than that they are founded on artificial stars, we find that one principle pervades the examination conducted at Greenwich, Paris, Bordeaux, and Algiers. The several astronomers have determined what length of time is necessary to produce a blackened star disc of the same diameter from the same star with and without the screen. In this way it has been found necessary to expose for ten or eleven times as long with the screen before the object glass as without, and from this fact it has been inferred that the loss of light occasioned by the screen amounts to 2·5 or 2·6 mag. It is urged that if only two magnitudes were lost by obstruction, the necessary exposure would have been $(2·512)^2 = 6·3$, that required by the unobstructed object glass. Dr. Dunér remarks on this that those who have condemned the employment of the screens on these grounds have argued in a vicious circle, and to be logically correct it would be necessary to show that the intensity varies as the time of exposure or

$$it = \text{const.}$$

Against the accuracy of this law Dr. Dunér urges that reports of the observers themselves show three distinct proofs. In the first place (1) Dr. Donner states that only 0·58 mag. is gained by successively multiplying the length of exposure by 2·5; (2) that the Astronomer Royal proves that a gain of 1·7 or 1·85 mag. is secured by multiplying the length of exposure by 6·25; and (3) that MM. Henry have found that to obtain similar discs from stars of the 9·3 and 11·3 mag. the exposure has to be increased from 28sec. to 240sec. (1 : 8·6). These three experiments give instead of 2·512 respectively,

$$3·28,\ 2·69,\ 2·93.$$

results apparently incompatible with the formula

$$it = \text{const.}$$

MM. Trépied and Henry reply at length and effectively to these strictures. They do not regard 2·69 and 2·93 as differing so greatly from 2·512 but that the discrepancy may be fully explained by inaccuracy and paucity of observations. The Helsingfors result (3·28) they refuse to accept as unequivocal evidence in the face of established facts. The method of Dr. Donner consisted in comparing photographs of the Pleiades, taken with and without the screen, with the map of M. Wolf, and marking the number and magnitude of the stars which have black or grey images. This method, as already hinted, does not seem to be entirely free from objection. Admitting that the comparison of the images was made, as we are sure it was, with all the care possible, there is still room for the varying exercise of individual judgment as to what constitutes a black and what a grey image, and the final result is likely to be less exact than a process based upon rigorous measurement.

The method employed by Prof. Pritchard is, perhaps, as free as any from objection or misinterpretation. He exposed the plate for *equal times* with and without the screen, and then measured the diameters of the resulting star discs. If two discs, produced, one with, and one without the screen, were found equal in diameter, then the effect of the screen is equivalent in photographic action to the original difference of magnitude between the two stars. This difference of magnitude was determined by the wedge photometer, and the only exception

that can be taken to this determination is that the scale of the wedge photometer may not be accurately applicable. But here we have the distinct assertion of the Astronomer Royal, reiterated again by M. Trépied, that the Pritchard Argelander scales are in very satisfactory accord. This circumstance is the more gratifying for two reasons. First, because it is distinctly stipulated in resolution 19 (1889), "Chaque observateur devra s'attacher à obtenir sur ses clichés destinés au catalogue la grandeur 11·0 déterminée aussi exactement que possible *au moyen de l'échelle d'Argelander*." The maintenance, therefore, of the scale of Argelander becomes of paramount importance, and this one could scarcely hope to effect by means of the gauze screens. The second satisfactory point is, that Prof. Pritchard is endeavouring to secure uniformity in the photographed stars by distributing among the participating observatories small charts of particular regions of the sky on which are marked stars of the 9th and 11th magnitudes approximately. Naturally in the determination of the magnitudes of the stars on these charts, the scale of Argelander will be perpetuated, and inasmuch as the testimony of several astronomers is distinctly in favour of making use of these typical areas, it seems very probable that Argelander magnitudes will be prolonged in the catalogue work down to the faintest stars impressed.

NOTES.

THE summer meeting of the Institution of Mechanical Engineers will be held in Portsmouth, and will begin on Tuesday, July 26. The following papers have been offered for reading and discussion, not necessarily in the order here given:—On shipbuilding in Portsmouth dockyard, by Mr. William H. White, F.R.S.; on the applications of electricity in the Royal dockyards and navy, by Mr. Henry E. Deadman; description of the lifting and hauling appliances in Portsmouth dockyard, by Mr. John T. Corner, R.N.; description of the new Royal pier at Southampton, by Mr. James Lemon; description of the Portsmouth sewage outfall works, by Sir Frederick Bramwell, F.R.S., Past-President; description of the new floating bridge between Portsmouth and Gosport, by Mr. H. Graham Harris; description of the Southampton sewage precipitation works and refuse destructor, by Mr. William B. G. Bennett; description of the experimental apparatus and shaping machine for ship models at the Admiralty experiment works, Haslar, by Mr. R. Edmund Froude; description of the pumping engines and water softening machinery at the Southampton water works, by Mr. William Matthews.

THE half-yearly general meeting of the Scottish Meteorological Society was held at Edinburgh on Monday, July 18. The council of the society submitted its report; and Dr. Buchan read a paper on variation in the annual rainfall in Scotland since 1800.

THE Museums Association held its annual meeting in Manchester, at the Owens College, on July 5, 6, and 7, under the presidency of Prof. Boyd Dawkins, whose address we print elsewhere. Among those present at the meeting were Dr. Ward, Principal of Owens College, Prof. Flower, Prof. Miall, the Rev. Canon Hicks, Prof. Milnes Marshall, the Rev. H. H. Higgins, and Prof. Weiss. Mr. J. Willis Clark, the retiring president, was unfortunately prevented from attending. The following papers were read and discussed:—On the arrangement of botanical museums, by Prof. F. E. Weiss.—On the cultivation of special features in museums, by the Rev. H. H. Higgins.—Local museums of art and history, by the Rev. Canon Hicks.—On the Manchester Art Museum, by Mr. T. C. Horsfall.—On the preparation of picture catalogues, by Mr. Butler Wood.—On the colouring of the background of museum cases, by Mr. Edgar R. Waite.—On the best means of preserving vegetable structures, and on a collection illustrating the life-histories of the British Lepidoptera, by Mr. J. W. Carr.—On the exclusion of dust, by Mr. T. Pridgin Teale; and library and museum legislation, by Mr. E. Howarth. Mr. Pacy and Mr. Ogle, who had been deputed by the Libraries' Association to attend the meeting, took part in the discussion of the last paper. A Committee of the Museums' Association was appointed to confer with the Libraries' Association on the possibility of taking steps to improve library and museum legislation. Most of the members of the Museums' Association who took part in the discussion were of opinion that the restrictions at present placed upon the action of Town Councils with regard to libraries and museums were unnecessary and obsolete. The meeting was a very successful one, thanks to the energy and good management of Mr. W. E. Hoyle and Prof. Milnes Marshall. The reception accorded to the Association by the authorities of the Owens College was of the most cordial nature, and the Association is indebted to Dr. Ward and several of his colleagues for much kindness. It was agreed to hold the next annual meeting in London under the presidency of Prof. Flower.

MR. WILLIAM E. PLUMMER has been appointed by the Mersey Docks and Harbour Board, director of the Liverpool Observatory, in the room of Mr. J. Hartnup, deceased. Hitherto this Observatory has done little more than regulate chronometers required for the port of Liverpool, but we understand that the Observatory will now be reorganized and made to play a more active part in observational astronomy, and one worthier of the equipment of the Observatory and the generous support the board accord to it.

DR. W. H. INCE, Ph.D. (Würzburg), Demonstrator of Chemistry in University College, Liverpool, has been appointed Demonstrator of Physics and Chemistry in the Medical School of St. Thomas's Hospital.

MR. A. H. LEAHY, M.A., Fellow of Pembroke College, Cambridge, has been elected to the Professorship of Mathematics at Firth College, Sheffield. Mr. Leahy is a Mathematical Lecturer and Junior Bursar of his College, and is the author of several important papers on mathematical physics.

MR. R. ELLIOT STEEL, Senior Science Master of the Bradford Grammar School, has been appointed by the Technical Instruction Committee of the Corporation of Plymouth to the Head Mastership of the Science Department of their new technical schools, Plymouth.

THE Master and Wardens of the Drapers' Company of the City of London recently gave £3000 towards the erection of the new technical schools attached to the Nottingham University College, and have now given a further sum of £1000 towards their equipment.

"COOK'S TOURS" are well known all over the civilized world, and vast numbers of Englishmen have been indebted to them for some of the brightest and pleasantest experiences of their lives. Everyone, therefore, was sorry to hear of the death of Mr. Thomas Cook, the founder of the system. He died at Leicester on Monday in his eighty-fourth year. Mr. Cook was a man of immense energy, and may almost be said to have had a touch of genius. At all events he had a very remarkable faculty for organization, and did much to foster among the British public a just appreciation of the advantages to be derived from foreign travel. Last year the jubilee of his firm was celebrated.

THE volcanic forces of Mount Etna have continued in a state of violent activity. On the afternoon of July 14 it was stated,

in a Reuter's telegram from Catania, that there were then eighteen openings in the mountain, of which nine were active. "The lava," said the writer, "is flowing in the direction of Nicolosi at the rate of 50 yards an hour. It has already passed the deposit of lava formed by the eruption of 1886. The flow towards Pedara is less rapid. Every hour the devastation increases, and the alarm of the inhabitants grows in proportion. Their terror is not lessened by the explosions and rumblings proceeding from the volcano." On July 15 a Reuter's telegram from Catania stated that the eruption was that day more formidable than ever. "The main crater is extending in size, and the showers of stones and masses of molten matter are continually increasing in volume, some of the projectiles being carried to a height of 1000 feet. Meanwhile, two fresh cones, 800 feet in height, have been formed, and from these streams of lava are constantly flowing in the direction of Nicolosi, from which they are now only about two miles distant. No immediate danger threatens the inhabitants of the village, but the destruction caused to the surrounding country goes on increasing." On July 16 and 17 telegrams to a like effect were despatched. On the latter date, indeed, it was stated that the eruption had been less active on the previous night, and the reports of the internal explosions less frequent and not so loud; but the volcano continued to throw up enormous blocks of incandescent rock together with clouds of steam. The lava stream had reached the village of Venatura, where it had destroyed several houses, besides doing enormous damage to the adjacent chestnut woods. On the 18th it was announced from Catania that during the previous night loud rumblings had continued, and that the discharge from the craters of Mount Etna had increased in violence, stones and ashes being projected to a height of over 1200 feet. In the morning the subterranean noises were less frequent and not so loud. At Patagonia, besides the volcanic explosions proceeding from Mount Etna, subterranean rumblings had been heard, while in the neighbouring naphtha lake, and the fountains of Vachella, gaseous eruptions had occurred. On the 19th, although the smoke proceeding from the craters was less dense, the eruption continued with renewed violence. The subterranean rumblings were more frequent and of longer duration, but not so loud as during the previous days.

ON July 15 it was announced from Naples that Mount Vesuvius had become active, and that lava in large quantities was pouring down the part of the mountain called the Atrio del Cavallo.

DURING the latter part of last week the weather over these islands was much disturbed by the influence of a deep depression which lay over the Baltic. The temperature was below 60° in the northern and below 70° in the southern parts of the kingdom, and the rainfall exceeded an inch in the south of Ireland. At the close of the week another depression appeared over the Bay of Biscay, and spread over our southern districts, accompanied by rain, while owing to the northerly winds the temperature continued very low, the maxima scarcely exceeding 60° in any part. In London on Sunday it did not exceed 55°, which, with about one exception, is the lowest daily maximum in July during the last half-century. During Monday night a deep depression advanced over Scotland from the northward, and travelled south-eastwards, accompanied by heavy rain, while on Tuesday increasing winds or gales were experienced on all our coasts, the wind direction varying from N.E. to N.W. and W. According to the *Weekly Weather Report* the rainfall for the week ending the 16th instant was considerably less than the mean in all the northern districts, while over the eastern, central, and southern parts of the kingdom there was a considerable excess, the amount being in many cases more than double the mean for the week. Temperature was below the mean in all districts excepting the Channel Islands; in the eastern and central parts of England the deficiency for the week amounted to from four to six degrees.

LAST week two despatches were received at the Colonial Office from Mr. Jerningham, Acting Governor of Mauritius, relative to the recent hurricane there. Mr. Jerningham states that the lives lost through the disaster were 1230, and the number of wounded still living 3167. Over sixty-two churches and chapels had been damaged or wrecked, and there had been a partial and enforced cessation of the celebration of Divine service throughout the island. The number of public buildings injured was 123, and the damage done to Government property was estimated at 286,807 rupees. The injury to the railways would cost about 55,435 rupees to make good. All the telegraph wires throughout the island were destroyed. About 16,976 houses and huts had been destroyed or damaged, exclusive of those in Port Louis, and about 170 sugar factories had been wrecked or injured. The task of repairing these disasters was one of great magnitude, and wholly beyond the unaided power of the colony. A later despatch states that in Port Louis 1453 houses, churches, and public buildings, representing a value of nearly five million rupees, had been wholly or partially destroyed.

THE *Kew Bulletin* for May and June contains several contributions which will be of great interest to botanists and to various classes connected with the industrial applications of botany. One of these contributions is a valuable report (with a plate) by Mr. George Massee on a disease that has attacked vanilla plants in Seychelles. In the same number are printed the second of the Decades Kewenses Plantarum Novarum in Herbario Horti Regii conservatarum, and the second decade of new orchids. An excellent illustration of the way in which the authorities at Kew seek to promote industry is afforded by a correspondence on Sansevieria fibre from Somali-land. The increased attention devoted to the production of white rope fibres in the Western tropics appears to have had a stimulating effect in the East Indies, and now the production of fibre from *Agave vivipara* in Bombay and Manila is followed by a fibre obtained from Somaliland from a singular species of Sansevieria. This fibre was first received in this country as an "Aloe" fibre. It was soon noticed, however, that it possessed characteristics differing from all ordinary "Aloe" fibre, and a request was made to the Foreign Office that Colonel Stace should be invited to obtain for the Royal Gardens a small sample of the fibre, a large leaf from the plant yielding it, and, if possible, a few small plants for growing in the Kew collection. In due time the specimens arrived in excellent order, and it was found that the fibre is one of the many so-called Bow-string Hemps, and probably yielded by *Sansevieria Ehrenbergii*, a plant first collected by Dr. Schweinfurth. Little or nothing was known of it until it was described by Mr. J. J. Baker, F.R.S., in the Journal of the Linnean Society, vol. xiv., p. 549. Its locality is there stated as "between Athara and the Red Sea." The plant is described in a letter to the Foreign Office, written by Mr. D. Morris, as a very interesting one, and he adds that its existence as a source of a valuable supply of fibre will be sure to awaken attention among commercial men in Great Britain. Messrs. Ide and Christie, writing to Mr. Morris, speak of the fibre as an excellent one of fair length and with plenty of "life." "In character," they say, "it strongly resembles the best Sisal hemp, with which we should have classed it but for your statement that it is derived from Sansevieria. With the exception of its colour, its preparation is perfect, and even as it is, we value it to-day at £25 per ton. We are of opinion that if care were taken to improve the colour a considerably higher price would be readily attainable, perhaps as much as £50 per ton, if a pure white fibre could be attained without loss of strength and lustre."

AMONG the other contents of this number of the *Kew Bulletin* is an account of the fibre industry in the Bahamas, communicated to Kew by Sir Ambrose Shea, Governor of the Bahamas. Extracts from a report by Mr. A. White, a naturalist attached to the staff of Mr. H. H. Johnston, H.M.'s Commissioner and Consul-General for the territories under British influence to the north of the Zambesi, throw welcome light on the botany of Milanji in Nyassaland. Mr. N. E. Brown contributes notes on the botany of plants yielding Paraguay tea. There are also sections on the Nonnen pest in Bavaria, the prickly pear in Mexico, and the Palmyra bass fibre.

THE collection of hardy bamboos and allied plants having outgrown the space allotted them in the beds near the Temperate House of the Royal Gardens, Kew, a new garden has been made for them in a wood near the Rhododendron Dell. Of this garden the *Kew Bulletin* gives the following account :—It is in the form of a shallow depression with sloping banks 12 feet wide and a central pear-shaped bed 125 feet by 75 feet. To make it, the surface soil had to be removed and the gravel taken out to a depth of about 3 feet. A large quantity of new soil and manure was added so that the bamboos have now a good depth of rich soil. Two new paths leading to the Bamboo Garden have been made, one from the Syon vista and the other from the Stafford walk. The bamboos planted in the garden are :—*Arundinaria Fortunei* (Bambusa Fortunei), *A. japonica* (Bambusa Metake), *Bambusa albo-striata*, *B. gracilis*, *B. nana* (Hort), *B. palmata*, *B. plicata*, *B. pumila*, *B. tessellata*, *B. Veitchii*, *Phyllostachys bambusoides*, *P. nigra*, *P. Quilloi* (Bambusa Quilloi), *P. violescens* (Bambusa violescens), *P. viridi-glaucescens* (Bambusa viridi-glaucescens), *Thamnocalamus Falconeri* (Bambusa Falconeri), and several others unnamed. Besides bamboos it contains such plants as *Arundo*, *Eulalia*, *Crinum*, *Funkia*, *Yucca*, &c. It is also intended to bring together in this garden a number of the coarser growing monocotyledonous plants which can be grown in the open air at Kew.

ACCORDING to an official "Notification of the Trustees of the Schwestern Fröhlich Stiftung" at Vienna, certain donations and pensions will be granted from the funds of this charity this year in accordance with the will of the testatrix, Miss Anna Fröhlich, to deserving persons of talent who have distinguished themselves in any branch of science, art, or literature who may be in want of pecuniary support, either through accident, illness, or infirmity consequent upon old age. The grant of such temporary or permanent assistance in the form of donations or pensions is, according to the terms of the foundation deed, primarily intended for Austrian artists, literary men, and men of science, but foreigners of every nationality, English and other, may likewise participate, provided they are resident in Austria. Particulars may be obtained at the Austrian Embassy, London.

MR. T. S. SHEARMEN, of Brantford, Canada, has recently issued a pamphlet, in which he claims priority in the discovery of the fact that the influence of sun-spots on terrestrial magnetic conditions depends upon the positions of the spots on the sun's disc as seen from the earth. He states that he has succeeded in convincing Prof. Young that this claim is justified. His observations have led him to believe that, in the great majority of cases, magnetic disturbances are most numerous when spots are at or near the eastern limb. In many cases, however, especially when the spots were very large, the disturbances have been greatest when the spots were near the central meridian; but even then it is stated that on nearly every occasion in which this has happened, another spot was making its appearance on the eastern limb. M. Veeder (NATURE, vol. xlvi. p. 29) also concludes that in order for a solar disturbance to have its full magnetic effect upon the earth, it is necessary that it should be at the sun's eastern limb, and as nearly as possible in the plane of the earth's orbit.

IN the tenth annual report of the Fishery Board for Scotland a striking instance is given of the advantage which persons engaged in the fishery industry derive from the electric telegraph. The Orkney officer reports that on Saturday morning, August 22, a large shoal of herrings was discovered about three to seven miles off the island of Stronsay by a few boats which happened to be at sea. Having ascertained the position of this shoal the officer wired the particulars, for the fishermen's information, to all the stations in Orkney. On the Monday following every boat employed in the herring fishery in Orkney was on the fishing ground indicated, with the result that the heaviest fishing ever obtained in one day in Orkney (for the number of boats employed) was landed on Tuesday, the average catch for the whole fleet being fifty crans. The number of boats fishing was 108, and their total catch was 5400 crans, valued at £3240, a large proportion of which would have been lost but for the telegraph. Wick fishermen having also been apprised of the circumstance, a number of the Caithness boats had good takes on the same ground and landed them at Wick. Consequent upon such a heavy and unexpected fishing, additional coopers, gutters, packers, barrels, and salt had to be immediately sent for from Wick so that the herrings might be cured while they were in a fresh state, and this was accomplished by means of the telegraph.

AN interesting exhibit of tobacco will be sent from Kentucky to the Chicago Exhibition. There will be exhibits of different varieties of plants in various stages of growth, and illustrations of the manner of shipping and handling "the weed" from the time the seed is put in the ground until the final product is ready for use. The various ways in which tobacco is used in manufacture will also be illustrated.

DURING the last few years much has been said about the supposed European origin of the so-called Aryan race. The honour of having first suggested this theory is usually attributed to Dr. Latham, but, according to Dr. D. G. Brinton, it really belongs to Omalius d'Halloy. In *Science* (June 24), Dr. Brinton refers to a paper in the *Bulletins de l'Académie Royale de Belgique*, tome xv., No. 5, May, 1848, entitled "Observations sur la distribution ancienne des peuples de la race blanche," in which Omalius begins by speaking of a series of notes presented by him to the Academy from 1839 to 1844. In these notes he had sought to prove that the Asiatic origin of the white race had never been demonstrated. Having recorded this fact, he proceeds "to discuss the evidence, physiological, historical, and linguistic, which had been thought to show that the Indo-European peoples originated in Asia; and combats it at every point, marshalling his arguments to prove that the true white type is distinctly European; and that the ancient Sanscrit and Zend are in no wise maternal languages of the Indo-European stock, but merely sisters of the Greek, Latin, and ancient German." The earliest date at which Dr. Latham expressed similar views was 1851.

SOME suggestive notes on Fuegian languages, by Dr. D. G. Brinton, were read lately before the American Philosophical Society. He refers to a very early Fuegian vocabulary, collected by the French navigator, Jouan de la Guilbaudière, during a sojourn of eleven months in the Straits of Magellan during the year 1695. It includes about three hundred words and short phrases, and no part of it has been published. The MS. copy of it in Dr. Brinton's possession he owes to the courtesy of M. Gabriel Marcel, the Librarian of the Geographical Section of the National Library of France. As M. Marcel intends to give it

publicity in the Compte-rendu of the Congress of Americanists, Dr. Brinton contents himself with illustrating its character by a limited selection of words. These show that the basis of the tongue is Alikuluf, and it differs, he says, scarcely more from the Alikuluf of the present generation than do between themselves the vocabularies of that tongue by Fitzroy and Dr. Hyades in the present century. A few words belonging to the Tsoneca and the Yahgan may be detected, probably introduced by trading natives.

THE new number of the Journal of the College of Science, Imperial University, Japan (vol. v., Part 1), contains studies on reproductive elements, by C. Ishikawa; further studies on the formation of the germinal layers in Chelonia, by K. Mitsukuri; papers on the development of *Limulus longispina*, and on the lateral eyes of the spider, by Kamakichi Kishinouye; a paper on the formation of the germinal layers in Petromyzon, by S. Hatta; and notes on a collection of birds from Tsushima, by I. Ijima. The papers are most carefully illustrated.

WE have also received the second part of the first volume of "Iconographia Floræ Japonicæ," by Ryōkichi Yatabe (Tokyo: Z. B. Maruya and Co.). The work consists of descriptions, in Japanese, of plants indigenous to Japan, with figures.

MR. T. E. BUCKLEY contributes to the current number of the Annals of Scottish Natural History some interesting notes on the vertebrate fauna of Sutherland and Caithness. The object of the notes is to bring the fauna of these two countries up to date. One bird, the ruff, is new to the Sutherland list, and Mr. Buckley is able to show the spread of certain other species such as the stock dove, tree pipit, &c. Eagles still hold their ground fairly well, but other birds of prey show a decrease. This, the author thinks, is only what might be expected, but it is sad, he says, to see how the hen harrier is rapidly approaching extermination. Plantations are growing up, and increase the number and breeding areas of certain species. When staying at Badenoch, he has been repeatedly struck in the autumn with the attraction which a few (say three or four hundred) small firs, a garden, and an acre or two of cultivated ground, have for migrating birds. Constantly in the early October mornings he has seen flocks of small birds, such as greenfinches, chaffinches, &c., descend into these trees, rest for a short time, then, with an unanimous twitter, rise up and pursue their onward course. As a rule everything was quiet for the day by nine o'clock.

AT the meeting of the Field Naturalists' Club of Victoria on May 9, Mr. T. S. Hall read an interesting note on musical sands. While on a trip to Phillip Island at Christmas time, Mr. Hall was struck by the musical note given out by the sea sand when walked over. He had never noticed this phenomenon before, though it occurs not uncommonly in other parts of the world. His first idea was that the sound was caused by the india-rubber soles of his shoes, but he found he could get the musical note by striking the sand with his hand, or by drawing a stick rapidly over the surface. The sound was produced only where the sand was dry, and resembled almost exactly that caused by drawing the finger rapidly over a piece of corded silk. On making the sound by skating over the surface, he found that the note could be detected at a distance of forty paces. The sands were musical wherever he tried them about Cowes, and the only person to whom he spoke who had noticed the phenomenon said he had also noticed it at San Remo. Mr. Hall has since tried the sand at Geelong, Barwon Heads and Warrnambool without any result. He referred to the theories of Mr. Carus-Wilson on the one hand, and Dr. A. A. Julien and Prof. H. C. Bolton on the other, and expressed a hope that some attention would be given to the subject in Australia.

IN the latest quarterly statement of the Palestine Exploration Fund it is said that considerable progress is being made with the Akka-Damascus Railway, the route of which, after various expensive surveys, has been definitely decided upon. The line chosen is practically that first suggested by Major Conder, R.E., several years ago. Beginning at the great fortress of Acre, the railway will run down the plain of Acre parallel with the sea, throwing out a branch to Haifa, at the northern foot of Mount Carmel, and thence to and across the plain of Esdraelon, passing near Nazareth to Shunem and Jezreel, and through the valley of Jezreel, skirting the slope of the hills, to the River Jordan, which will be crossed within sight of Bethshean. The Jordan here offers exceptional facilities for the erection of the railway bridge, consisting of two spans. Not only are the two opposite banks of the river formed of solid rock, but the centre of the river contains a large block of similar rock, from which each span of the bridge will be thrown to the east and west bank respectively. From the Jordan the railway will ascend the slope of the Jaulan Plateau, along the crests that close the eastern shores of the Sea of Galilee, this ascent constituting the only difficult portion of the line, but which the surveys now made show to be much easier of accomplishment than was originally anticipated. The plateau near El 'Al being reached, an easy gradient will carry the line by Seil Nawa and Kesweh to Damascus. Passing through the finest plains of Western and Eastern Palestine, the railway will be one of great importance. The authorities of the Palestine Exploration Fund are of opinion that its construction can hardly fail to lead to important archæological discoveries, and the committee hope to make arrangements for obtaining full information respecting these.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Major Day; two Red-handed Tamarins (*Midas rufimanus*) from Surinan, presented by Mr. J. J. Quelch, C.M.Z.S.; two Sœmmerring's Gazelles (*Gazella sœmmerringi* ♂ ♀), three Egyptian Gazelles (*Gazella dorcas* ♂ ♀ ♀) from Suakim, presented by Colonel Holled Smith, C.B.; a Red Deer (*Cervus elaphus*), European, presented by Mr. J. Newton Hayley; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, presented by Mrs. Duppa; a Rough-eyed Cayman (*Alligator sclerops*) from South America, presented by Dr. Rudyard; two Dwarf Chameleons (*Chamæleon pumilus*) from South Africa, presented by Mr. E. Windgate; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. J. Cornwall; two Green Lizards (*Lacerta viridis*), two Green Tree Frogs (*Hyla arborea*), European, presented by Count Pavoleri, F.Z.S.; a Horned Lizard (*Phrynosoma cornutum*) from Texas, presented by Mr. Conrad Kelsal; a West African Python (*Python sebæ*) from West Africa, received in exchange; six Mandarin Ducks (*Æx galericulata*), five Summer Ducks (*Æx sponsa*), seven Chilian Pintails (*Dafila spinicauda*), six Australian Wild Ducks (*Anas superciliosa*), a Variegated Sheldrake (*Tadorna variegata*), four Upland Geese (*Bernicla magellanica*), a Cheer Pheasant (*Phasianus wallichii*), Himalayan Monaul (*Lophophorus impeyanus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

A NEW NEBULOUS STAR.—Mr. Barnard, in *Astronomische Nachrichten*, No. 3101, gives a brief account of a new nebulous star that he found when photographing, on May 31 last, a region situated in the Milky Way, 18h. 10m., $-20°$. This star (B.D. $-19°4953$), when examined (visually) with his 12-inch, was quite devoid of nebulosity owing to the brightness of the star in question, but the photograph showed a faint nebulosity of about 15′ in diameter symmetrically surrounding it. A

former photograph taken in 1889, July 28, indicated also the same nebulosity. The exposure in the former case was of 3h. 29m. duration, a Willard lens of 6-inch aperture being used. The position of the star for 1860 was R.A. 18h. 9m. 23·2s., Decl. − 19° 42′·7.

ATMOSPHERIC DEPRESSIONS AND THEIR ANALOGY WITH THE MOVEMENTS OF SUN-SPOTS.—The solar photosphere, although so different in chemical composition from our own atmosphere, yet affords us many points of resemblance with regard to its general circulation. One special analogy, and that a comparison of the motions of storms here with those of spots on the solar surface, is treated of in the July number of *L'Astronomie* by M. Camille Flammarion. In this article he has brought together sufficient observations to trace out the paths of many of the most violent storms that have from time to time visited Europe generally. The first storms which he gives are those which occur in the Atlantic; their general direction of motion seems to be from south-west to north-east, pursuing generally the path of the Gulf Stream. Their centres, when traced on a map, seem to just graze the shores of the British Isles, France very rarely being reached by them. From observations made on land, and more especially from those at Paris, M. Flammarion remarks that certain curves with regard to these storms seem to offer many analogies to solar spots; this is so not only for the regular displacements, but even for those which at first sight seem to be totally void of all regularity. The diagrams which he gives, showing both the paths of the storms and those of sun-spots, afford most interesting comparisons and seem to confirm the view suggested by M. Faye that the constitution of spots resembles somewhat that of the cyclones with which we are familiar.

YALE COLLEGE OBSERVATORY REPORT.—In this report, submitted by the Board of Managers to the President and Fellows of the Yale Observatory, Mr. Brown makes us acquainted with the present condition of the Observatory generally, while Dr. Elkin gives an account of the work done by the heliometer during the past year. The satellites of Jupiter formed the principal object of work from July 1891 to January 1892, 570 complete measures of their relative positions having been obtained on 114 nights. Dr. Chase, with the same instrument, has been measuring the cluster in Coma Berenices, securing from 18 to 20 measures for each of the 32 stars, besides determining the required data for the reduction-constants. In the work on the parallaxes of the first-magnitude stars in the northern hemisphere, the 100 sets of measures of each of the ten stars have not yet been fully completed, but the following table shows the results obtained up to the present, Dr. Elkin thinking that it will require two more years before the final results can be published:—

Star.	Parallax.	Prob. Error.	No. of comp. stars.	No. of Sets.
	″	″		
α Tauri	+ 0·101	+ 0·022	6	65
α Aurigæ	+ 0·095	0·021	5	51
α Orionis	+ 0·022	0·022	6	48
α Canis Minoris	+ 0·341	0·020	6	48
β Geminorum	+ 0·057	0·021	6	48
α Leonis	+ 0·089	0·026	10	43
α Bootis	+ 0·016	0·018	10	89
α Lyræ	+ 0·092	0·019	6	67
α Aquilæ	+ 0·214	0·023	10	46
α Cygni	+ 0·012	0·020	7	49

GEOGRAPHICAL NOTES.

THE condition of affairs in Uganda, of which much has recently been made in home and foreign newspapers, is a question rather of politics than of geography; yet problems of a geographical kind are involved in it. So far the progress of civilization amongst the Waganda has served only to introduce new elements of dissension, and the attempts to carry out the policy of preventing the sale of spirits, firearms, and ammunition have only been partially successful. Scientific exploration in a country so unsettled must necessarily be suspended. If the British occupation is to be productive of benefit either to British trade on the one side or to native interests on the other, the firm and impartial rule of Captain Lugard and Captain Williams must be maintained and reinforced. The urgent request which these officers have made for additional white assistance demands all the more attention since the garrison under their control has been swelled by the survivors of Emin's force in Equatoria.

ALTHOUGH, as announced in NATURE (p. 230), Captain David Gray's Antarctic expedition has fallen through, it is satisfactory to know that three Dundee whalers, which are shortly expected back from the Arctic "fishing," will be refitted immediately and despatched about the beginning of September to the Falkland Islands, and as far to the south as may be necessary in order to get a cargo. The proposed cruise is to be purely commercial, and it is not likely that any exploring work will be done. It is probable that berths will be available on board for two or more scientific men, who should have good opportunities of collecting natural history specimens. The experience of the whale-ships will be valuable in supplying hints for the equipment and route of the great Antarctic expedition which under some European flag cannot be long delayed.

DR. OSCAR BAUMANN, charged with the survey of a road to the Victoria Nyanza, reached the shores of that lake in April, after an unprecedentedly rapid journey from the coast. From the *Times* report of a letter written by Dr. Baumann from Kadoto, we learn that the route, after passing around Lake Manyara, struck across an unknown stretch of country in which a new lake of large dimensions was discovered. Even in Africa few lakes of such magnitude can now remain unknown, at least from native reports. Lake Eiassi lies on the plateau south-east of Victoria Nyanza, and from the report of the neighbouring Masai, it seems to be about ninety miles long, while the breadth of the northern portion, along which Dr. Baumann marched, varied from eighteen to thirty miles. This lake is presumably filled with fresh water, but no outlet is mentioned. It is interesting, however, to find native reports of a great river flowing in on the western side, which may be confidently identified with the Wemberi, a river shown on recent maps as flowing north-eastward from the border heights of Unyamwesi, and losing itself on the plateau. It is possible that the new lake may discharge into the Victoria Nyanza by the Simiu river, the head waters of which have not previously been explored.

THE recently founded New Zealand Alpine Club has published the first number of its *Journal*, devoted to the exploration of the glaciers and peaks of the Southern Alps. The magnitude and difficulty of these snow mountains of the south has hitherto been very inadequately realized.

THE MUSEUM QUESTION.[1]

GENTLEMEN of the Museums Association,—In taking the chair which was so ably filled by my predecessor at Cambridge, I must first of all give you a hearty welcome on the part of the Committee of the Manchester Museum. There is to my mind a singular fitness in the selection of Manchester as a place of meeting after Cambridge. At Cambridge you had the opportunity of studying the various museums which have in the course of time naturally grown out of the development of that ancient seat of learning. In Manchester you will see the collections which have been gathered round Owens College, which represents the newest University development in this country. The genius of the place has left its mark in both. In Cambridge the collections are, as they should be in a region of academic calm, free from trade-winds, arranged mainly, if not entirely, with an eye to University students, and not for the general purposes of a miscellaneous public. In this busy centre of movement and commerce, you will find that the principle of arrangement is twofold. It first aims at meeting the needs of the University students, and of the Mechanics' Institutes, and schools, and other educational bodies, which are daily being drawn closer to Owens College, and next at the instruction and enjoyment of the general public. Our collections are for the most part older than the college and have been absorbed from without into our educational centre.

The problem which we have attempted to solve is this: How to arrange and organize collections which are in part as old as the second quarter of this century, so that they may become valuable in the new learning and at the same time put an outline of the history of nature within reach of the people. This

[1] Address to the Museums Association, Manchester meeting 1892, by the President, Prof. Boyd Dawkins, F.R.S.

problem, as we all know, is not an easy one. It has, however, to be faced in most of the museums of this country. Whether it has been solved or not in Manchester, it is not for me to say. Prof. Huxley, in addressing us some years ago on the question of technical education, said that he did not know exactly what shape it should take, but that Manchester was a good place in which to make an experiment for the good of the Commonweal. *Fiat experimentum in corpore Mancunensi.* The result of our experiment in museum organization is here and speaks for itself.

Before, however, I deal with this special attempt to meet the needs of Manchester I must touch on the general question of museums.

The Museum Idea and its Place in Culture.

A museum was to the Greeks a place haunted by the Muses, and in a secondary sense a building in which liberal studies were carried on such as that at Alexandria, which was a great University endowed by the State, divided into colleges, and frequented by men of science and letters. This museum included picture galleries and statuary, and it is not at all improbable that it contained also collections of Natural History. Aristotle, it must be noted, made vast collections, which he used in his history of animals, by the aid of his friend Alexander the Great, and it is hard to believe that the impulse which he gave to the study of natural history should not have been felt in Alexandria, where his memory was venerated. With the destruction of this museum in the days of Aurelian in the last quarter of the third century after Christ, the name as applied to a public institution gradually dropped out of use, and was only revived with the revival of learning in the West in the times of the Renascence. We owe the first idea of a great national museum of science and art to the "New Atlantis" of Lord Bacon, the first scientific museum in this country to Elias Ashmole, who founded the museum which bears his name in 1667 at Oxford. It consisted mainly of the natural history collections made by the Tradescants, and miscellaneous objects of antiquarian interest, which in the course of time swamped the natural history. Now, under the care of Mr. Arthur Evans, reorganized and rearranged, it is taking its place among the educational institutions of the University. It was not until 82 years after the foundation of the Ashmolean that museums were recognized by the Government of this country in the establishment of the British Museum in 1749 by Act of Parliament.

The modern museum is the outcome of the Renascence, and must keep pace with those great accessions to our knowledge of the history of Nature and of Man which distinguish the new from the old learning of to-day. If it cease to grow it is dead, and should be removed. There is no finality in museum work any more than there is finality in the acquisition of knowledge.

The Muses should not be forgotten in museum arrangements, and form, beauty, and symmetry should be studied as well as the outlines of a rigid classification. As an illustration of this I would refer to the groups of birds in the Natural History Museum at South Kensington, or to some of those in the museum at Newcastle. "A thing of beauty is a joy for ever." There is no reason why things beautiful in themselves should be treated so as to repel rather than attract. The element of fit association, too, is one of the important principles. In the case of the temporary exhibition of pictures in the Academy, the fit association of subjects cannot be studied, but there is no reason why in an art gallery the Muses should be scared by a Venus being placed close to local worthies, or a Madonna close to a party of Bacchanals. In museums as in armies the results are largely dependent on the leaders, who leave the impress of their personal character on their commands. The impulse to the new learning given by Ashmole, Hunter, Flower, and by Franks and Newton in this country, by Leidy, Dana, Marsh, and Agassiz in the United States, will last as long as the museums which they founded and organized. In Paris the name of Cuvier is inseparably bound up with the Museum of Natural History in the Jardin des Plantes. In Berlin the Ethnological Museum, and the anatomical collection at the Charité, will keep fresh the memory of Bastian and Virchow in a far distant future when the names of the great political leaders of these times are fading away. It is, therefore, of primary importance to choose good leaders for museum work, and to offer those inducements which will command the services of the best men.

The Old Local Museums in Britain.

When I first began to study the question, some thirty years ago, most of the local collections in this country were in a deplorable state. They consisted largely of miscellaneous objects huddled together with more or less care, and more or less—generally less—named. In one you saw a large plaster cast of a heathen divinity, surrounded by stuffed crocodiles, fossils, and models of Chinese junks, which looked like the offerings of devout worshippers. In another I remember a small glass case containing a fragment of human skull, labelled "human skull," and a piece of oatcake, labelled "oatcake," while underneath was a general label with the inscription, "A piece of human skull very much like a piece of oatcake." In a third wax models were exhibited of a pound weight of veal, pork, and mutton chops, codfish, turnips, parsnips, carrots, and potatoes, which must have cost the values of their originals fifty times over. They had labels explaining how much flesh and fat they would make—theoretically—for we who are either lean or fat know that the personal equation has to do with the actual results. They were as carefully modelled as the most delicate preparations of human anatomy. I quote them merely as illustrations of the misapplication of time, money, and museum space.

In many collections art was not separated from natural history, nor from ethnology, and the eye took in at a glance the picture of a local worthy, a big fossil, a few cups and saucers, a piece of cloth from the South Seas, a model of a machine, and probably also a mummy. These objects would be all very well in their places, but being matter in the wrong place, they were covered by the Palmerstonian definition of rubbish. Such collections as these neither please nor instruct. They have no more right to the name of a museum than a mob has to be called an army. Most of us, I think, are acquainted with this type of collection, which is rapidly becoming extinct with the spread of knowledge. I merely quote them as examples of a state of things from which we have fortunately escaped.

The Place of Museums in the New Learning.

The rapid increase of knowledge makes it more and more necessary for museums to be organized, so as to be in harmony with the swiftly changing conditions. The study of things as well as books is daily growing in importance. The historian, for example, formerly content with written records, now counts the results of archæological discovery among the most valuable and trustworthy of his materials in dealing with the history of the past. The story of Ancient Greece is incomplete if the explorations at Mykenæ and Ilios, in Athens, in the Greek islands, and in the Egyptian cities and tombs be left out of account. To the historian the collections of Schliemann, Flinders Petrie, and others are most precious. Nor are they less precious to him who studies art, or to the ethnologist who studies civilization, or to the naturalist who is interested in the distribution of the various types of mankind, or to the technologist who looks to the evolution of handicrafts. The public mind is beginning to realize the value of well-organized museums for purposes of special research as well as of general culture, and thus they appeal to the interest of the many, while books and a taste for books interest a narrower circle. To contemplate in the British Museum the frieze of the Pantheon is of itself an education in Greek art and in Greek ideas of beauty, and the most unlettered visitor to the Natural History Galleries cannot fail to carry away new ideas about the realm of nature. It is obvious, therefore, that in museums we have an instrument of great educational value, if they be organized to meet the increasing demands of modern investigation.

The Classification of the Museums of To-day.

The museums of to-day fall naturally into four groups. (1) The Art Museum, which includes also antiquities arranged from the art point of view. (2) The Natural History, which illustrates the history of nature in its widest sense, and of man in his physical aspects. (3) The Archæological and Ethnological, which deals with the works of man and his progress in civilization. (4) The Technical, in which objects are arranged in relation to industry. The leading idea of the first is art, of the second nature, of the third civilization, and of the last the conquest of mind over matter.

The Principles of Museum Organization.

These four groups are sharply defined from each other. In practice however it is often necessary to use for the illustration of one what, strictly speaking, belongs to the others. In all such cases, however, the reason of the presence of the alien object must be made obvious, if the general effect of the arrangement is to be preserved. For example, in the Manchester Museum, I found it necessary to complete the history of the Tertiary Period to illustrate the first appearance of man, and to carry on the narrative through the prehistoric and historic divisions down to modern times by a small selected series of specimens, showing the progress of mankind. Were it not for this they would be wholly out of place in a collection of natural history. In like manner our Egyptian mummy has its due place in the National Gallery in Trafalgar Square, its *locus standi* consisting in the fact that it illustrates the art of portrait painting among the Alexandrine Greeks of the first century after Christ.

Next in the point of importance to the leading ideas in museum work comes the question of labelling and illustration. The labels should be clear and distinct, and if possible in English as well as in Latin. The specimens should illustrate the labels quite as much as the labels the specimens. All possible means of illustration should be employed, maps, diagrams, restorations, and the like, so that the main points and relations are clearly brought home to the visitor. In addition to the systematic catalogue of each specimen there should also be a popular guide similar to those of the British Museum. It goes without saying that a collection of books is also necessary.

The kind of museum most desirable in any place depends entirely on the local conditions, and there is no hard and fast scheme applicable to all cases. Nor is the question of great or small to be looked at otherwise than as one of detail. A small well-arranged collection in a school or in a village will do the work which it is intended to do as well as large museums in the metropolis, or in a university, or in a centre of commerce. The principles of success are the same in all: they must be orderly, they must be intelligible, they must as far as possible appeal to the sense of beauty. Under no circumstances must unnamed and unknown specimens be allowed to appear. A ragged recruit may be drilled into a good soldier, but he spoils the parade if he appears out of uniform in the ranks. Nearly all of us who have had to do with museums have sinned in this matter, and it is not for me to cast a stone at my fellow sinners.

The work, however, is only partially done when a museum is properly arranged, labelled, and catalogued on the above lines. To make it intelligible in the best possible way, it is necessary that there should be lectures and demonstrations given in the museum itself, in which some special points should be taken up which interest either the general public or the special worker. In my experience oral instruction with the things before the eyes in the museum, and not away from it in the lecture-room, is the best manner of doing this. As an example of this, I would refer to the demonstrations organized in the British Museum by Prof. Stewart Poole, in which ancient art and civilization were dealt with, and to those which have from time to time been given in the national collection of natural history, under the auspices of Dr. Flower. In this relation the British Museum will be found to be one of the most valuable instruments for spreading knowledge in the University which London will have in the future. In this relation, too, the Geological Museum in Jermyn Street, around which are centred some of the ablest men of the time — De-la-Beche, Murchison, Ramsey, Edward Forbes, Tyndal, Huxley, and many others—has done most valuable service. It is in this direction that museums will influence the general education in this country, and take their natural place in the new learning.

Application of these Principles to the Manchester Museum.

I pass now to the application of the above principles to the Manchester Museum, Owens College. Our experience gained in bringing the old collection into harmony with modern requirements cannot fail to interest those who are now engaged in like work, because it may show not only what is to be copied, but what is to be avoided.

When the task of organization was entrusted to me in 1869, there was a large general collection of natural history, and a large geological collection. The former had been a first-class collection in the second quarter of this century, but had ceased to grow, and therefore had become dead. The second was in good order, and, under the care of its founders, Binny, Ormerod, and others, was properly named. Both, however, were in a most deplorable state so far as relates to fittings, and were simply ignored by the general public, and scarcely used by students. The first step was to sweep out of the way the miscellaneous objects which had no place in a Natural History Museum. The next was to organise what remained into a systematic collection in rooms and cases which were unfit for the purpose. Then followed evening lectures and demonstrations in the old Museum building in Peter Street. Later the teaching collections in Owens College were added, and the Museum began to revive and grow, slowly but steadily, as the connection with the College grew closer, till, in 1874, it was transferred to temporary quarters in the attics and basement of the Owens College. It continued to grow in spite of the removal and of the inadequate cases, and the interest of the public was maintained very largely by the system of Saturday afternoon demonstrations in the only part open to the public—the Geological Museum.

The systematic rearrangement in view of the new buildings was taken in hand. The minerals were arranged, labelled, and catalogued, Dana's "Hand-book of Mineralogy" offering a ready-made catalogue. To meet the mining interests of Manchester special groups of the minerals found in association were organized to illustrate the minerals of Derbyshire, the Lake District, Cheshire, the diamond mines, the apatite mines, and the like.

For the special ends of the geological teaching, the rock specimens were also arranged, and special groups were formed to illustrate their association—such as the products of Vesuvius, and of the volcanoes of Auvergne—and to illustrate the destruction of rocks by natural causes. Then naturally followed the classification of the fossils to show the sequence of events in the geological record. In this the Carboniferous flora and fauna naturally took a prominent place, because of the vast importance of the coal-fields to this district. The arrival, too, of the existing higher Mammalia, including man, on the earth, took a prominent place in the Tertiary collections, and formed the leading idea in the Tertiary chapter of a history of the earth, while the story of the earth was fitly closed by a series of groups illustrating the evolution of human culture and the prehistoric and historic periods. The general principle of classification throughout the whole geological series, or, in other words, the historical method, was that of *time*. Next the zoological collections were arranged, as far as the changing classification would allow, zoologically, with a special group for the zoology of Great Britain. The botanical collections, which offered exceptional difficulty, are now in hand. In this manner the whole of the collections were arranged for the time when they should find their place in the new buildings, and pass under the care of the professor in each department. A scheme of uniformity was carried out with regard to fittings and mounts also; a definite unit of size, $4'' \times 1\frac{1}{2}''$, was decided upon, and all tablets and glass boxes were made either on that or on multiples of that. This unit also ruled the size both of the drawers and of the cases in the new fittings. The system of printed labels in which black ink represents the specific name and the red the name of the group was also devised. In the plans of the new Museum the maximum amount of light, consistent with stability and architectural beauty, was the leading idea, while the laboratories and lecture-room of the whole of the Natural History Department of the College were brought as close as they could be to the Museum. The building itself was designed to suit the organization of the collections. Thus step by step the present Museum was gradually built up, and when the buildings were completed in 1884 the collections were transferred to the quarters which they now occupy, and where they form a centre towards which other collections gravitate.

While the museum has been rapidly growing during the last eight years, the system of museum lectures and addresses to various organizations, mechanics' institutes, schools, and the like has been largely developed. In its present state it is used largely by students of Owens College, and is growing in favour with the general public. In other words it is taking the place it ought to have in the education of this densely populated district. These results, it must be observed, have only been possible through the liberality with which the Museum has been treated both by the public and by Owens College. I look forward with confidence to the time when both will be amply repaid by the impulse it is giving, and will give, to the new learning.

I do not for one moment suppose that a natural history museum of this kind is suitable for all places. The *genius loci* must be, in all places, the genius of the museum. The principles however of success are the same in all, and success can only be achieved in a limited degree if there be no signs of the worship of some of the Muses in the arrangements.

The Work of the Museums Association.

In ending this address, all too long, I fear, for my audience, all too short for my subject, I must add a few words as to our work as a Museum Association. It is twofold. First, we must arouse ourselves to the present situation and note the directions which the intellectual movement of the day is taking. Next, it is our duty to arouse the public to the importance of museum development, and to take care that the claims of museums as instruments of education shall not be ignored in the grants made by public bodies for the good of the commonweal.

ON THE CARBURIZATION OF IRON.

I.

THE conditions under which carbon combines with iron have been closely studied, and the observed phenomena fully discussed. Even now, however, it is doubtful whether true chemical combinations of carbon and iron are formed. It has been alternatively assumed that carbon is with difficulty soluble in iron, and that at low temperatures solution may proceed very slowly. In other words, carbon is not easily dissolved except at high temperatures; and it follows that if highly heated iron fully charged with carbon be cooled, a portion of the carbon must be precipitated in this state, existing simply as foreign matter in the metal, but that, on reheating, it may again enter into solution. Low carbon steels may be regarded as dilute solutions of carbon in iron; pig or cast iron as saturated; and intermediate grades may be termed moderately concentrated solutions.

Against this, however, there is a mass of evidence which deserves attention and cannot be ignored. Percy states that for the full carburization of iron a high temperature is necessary, and further, considering the absolute infusibility of carbon, it seems reasonable to assume that these elements must enter into chemical combination. It is, however, admitted that this compound may have the power of dissolving additional carbon; this explains the copious deposition of carbon in the graphitic form when iron is cooled. Dr. Percy finally concludes that there must be at least one definite compound of carbon and iron, but adds that there seems to be no reason why solution should not occur, as in the case of mercury, which liquefies gold, silver, or copper.

Prof. Roberts Austen also ("On Certain Properties common to Fluids and Metals," Royal Institution, March 26, 1886) speaks of the power which certain solid metals have of even rapidly taking up fluids—clearly cases of solution. Abel claims to have proved the existence of a definite compound of carbon and iron. Prof. Roberts Austen also finds that heated iron combines with pure carbon in the form of diamond dust. The author also has succeeded in directly combining iron fused *in vacuo* with pure sugar charcoal presumably freed from gases by repeated heatings *in vacuo*. Yet it is obvious these instances may all be explained on the theory of solution at elevated temperatures, with the exception of Prof. Abel's, who claims to have isolated a definite carbide of iron from the metal.

Matthieson, as the result of an elaborate research, states that "with few exceptions" most of the known two-metal alloys are solidified solutions of one metal in another. Carbon-iron alloys may be looked upon as solidified solutions of carbon in iron, and the analogy of cast iron with other alloys indicates the non-existence of chemical combination between carbon and iron.

Again, viewing alloys as definite chemical combinations, the facility with which heated iron absorbs certain gases does not admit of easy explanation.

Deville, however, imagines a kind of porosity in the metals, terming it an intermolecular porosity, sufficient to admit of the passage of gas at a low temperature; and supposes it developed by the expansive agency of heat. Graham assumes that the affinity of the gases for iron and platinum is as the attraction admitted to exist between a soluble body and its solvent.

Other metallurgists are of opinion that carbon does not directly combine with iron, attributing their union to the direct action of carbon monoxide gas always present in iron; by the agency of this gas carbon is indirectly transferred to iron; but it would appear that this cannot be maintained, for it has been proved that carbon combines directly with iron one way or the other, *i.e.* by solution or chemical combination.

Whatever may be said of irons containing an excess of carbon, *i.e.* cast iron and very hard steel—which, if one grants that carbon is not very soluble in iron at a low temperature may be termed supersaturated solutions—in the case of low carbon steels there seems some ground for assuming that carbon is merely dissolved in the metal.

Sir L. Bell tells us that, on heating thin sheets of carburized metal or steel piled closely together, the excess of carbon contained in one or more of the sheets is transferred to the others. Wrought iron is carburized in much the same manner by the cementation process, and it is equally possible that heterogeneous iron, *i.e.* iron containing intermixed carbon or graphite, and as a rule not equally diffused, may by continued sufficient heating become practically homogeneous.

It is a well-known fact that the carbon in low carbon steel—for instance, Bessemer steel—exists in at least two different forms; Prof. Ledebur says four. Akerman (Iron and Steel Institute) classifies these as (1) hardening carbon, or the carbon which determines the quality of steel, (2) cement carbon, and also graphite may be present.

The united researches of many workers in this field of research indicate generally that a portion of the total carbon is in intimate union with the metal, and that the more intimately combined or hardening carbon determines the quality of the steel. The carbon incompletely combined (or intermixed carbon) is termed cement carbon, because it occurs in the largest proportion in blister or cement steel.

Does not the above point to a case of solution of carbon, in which the quantity in solution is determined by temperature, just as with other solutions?

Metallurgists, however, can hardly accept the theory of solution without qualification.

Mr. Spencer states that "unhardened steel containing 1·18 per cent. total carbon—of which the colour test indicated ·89 per cent. as combined carbon, and residual carbon or graphite ·29 per cent.—after being hardened, gave only ·58 by colour test, and only traces of graphitic carbon, showing a loss of ·51 per cent. of carbon. A softer steel, containing ·50 total carbon—equalling ·45 per cent. by colour test, ·04 per cent. graphitic carbon—after hardening, only ·21 colour test carbon; graphitic carbon ·00, showing a loss of ·29 per cent. Other analyses were made confirming the above, and establishing the fact that after hardening there is always a proportion of carbon which can neither be determined as graphite or by the colour test, and this proportion is found to increase according to the larger amount of carbon in the metal, and the rapidity with which it was cooled" (Mr. Spencer, Iron and Steel Institute).

The facts above quoted are not apparently in accord with the theory of solution; but there are undoubted allotropic modifications of carbon, and this peculiar form may be one of these, "uncombined," and may be classified with the graphite, or really as merely intermixed foreign matter.

There is the alternative assumption that the missing carbon may exist in some form or combination with the iron, possibly not capable of being registered by the colour test; but as the steel is treated with dilute nitric acid, in which it is completely soluble, with the exception of the graphite, this assumption can hardly be maintained.

Referring to Akerman's assertion that only combined "hardening" carbon determines the physical properties of steel—an assertion with which Mr. Spencer agrees—"The apparent loss of carbon shown by the latter, and which we have determined as intermixed carbon or a form of graphite"—it may well be that the missing carbon is so intimately mixed as to be in a state closely bordering on solution, for it is well known that it is difficult to draw the line between absolute solution and matter finely suspended in a liquid. The latter practically often presents the appearance of a solution scarcely to be distinguished from it. Messrs. Harold Picton and L. E. Linder (Chem. Soc., January 1891) are of opinion that there is a continuous series of grades of solution passing without break from suspension to a crystallizable solution. This seems very probable, and in accordance with our chemical experience.

Graphite, if the author has adequately grasped Prof. Akerman's views, has little or nothing to do with the quality of

iron. "Graphite carbon exerts an influence only on iron in so far as it diminishes the continuity of the iron molecules. We often meet with the incorrect statement that the influence of carbon on pig-iron is quite different from its action on steel and malleable iron.

"It is easy to prove to the contrary if we distinguish properly in pig-iron between the combined carbon and that which is only mechanically incorporated as graphite, which ought not to be included in the calculation if we wish to form a judgment on the properties of pig-iron as dependent on its contents of carbon."

As one understands this, the same applies to steel.

So far there can be no difficulty in assuming at least the probability of the solution of carbon in iron, and that the physical qualities of the metal are determined by the quantity of carbon in solution, *i.e.* Akerman's hardening carbon.

The facts, *per contra*, appear mainly to indicate that carbon is merely sparingly soluble in iron at temperatures below its fusion-point.

A more serious objection (previously referred to) is that carbon is practically infusible, more especially in the graphitic form. How this intractable body so readily interpenetrates iron is a problem not easily solved.

The ordinary chemical theory of solution as usually understood does not, however, seem applicable on the whole; but some of the results accruing from the recent development of the gaseous, or rather physical theory of solution, may be made available for this purpose.

The Physical or Gaseous Theory of Solution.

In cases of simple solution the dissolved substance may be regarded as being evenly distributed throughout the solvent. The substance is dissolved by virtue of osmotic pressure, and Van 't Hoff has shown that osmotic pressure in solutions corresponds to gaseous pressure in space.

Further, it appears that both Boyle's and Charles's law holds good, at least for dilute solutions, osmotic being the equivalent for gaseous pressure, which pressure increases for constant volume proportionally to the absolute temperature. It has been, however, objected that Boyle's law is not strictly applicable to "more especially concentrated solutions," but Prof. Orme Masson (NATURE, February 1891), states that these are comparable with the case of gases at high pressures. Again, exceptions are claimed under the law of Avogadro, *i.e.* equal volumes of gases contain equal numbers of molecules under like conditions of temperature and pressure, but as regards compound gases exceptions occur, as also with dilute solutions.

Exceptions can be explained by the theory of dissociation. The analogy between gases and the physical theory of solutions thus seems complete, and Ostwald describes an experiment indicating the existence of free ions in a dilute solution of potassium chloride; other instances might also be quoted.

The author's object, however, is not to discuss the absolute correctness or otherwise of the theory of gaseous solution, which seems pretty well established; but to show that it may be applicable to the solution of carbon in molten, semi-fluid, or even merely heated iron, apart from possible cases of dissociation and chemical combinations. Solution is simply the even distribution of one body in another, or such distribution as that of permanent gaseous matter through space. It may be urged that the theory is not applicable to semi-fluid or merely heated iron. No definite line can, however, be drawn; it is obvious that the different grades of temperature are simply approximations, more or less, to the ideal fluid condition. The law of solution, as above defined, may suffer modifications, but need not in consequence be rejected.

"*Definition of Osmotic Pressure.*[1]

"Osmotic pressure is really a definite force. With suitable apparatus this force can be measured, in centimetres of a mercury column, and Pfeffer has shown that this, the osmotic pressure, is intimately connected with the nature of the dissolved substance.

"The pressure was found to be dependent on, and in proportion to, the concentration of the solution; the pressure at a specified concentration is dependent on the temperature—a rise in temperature corresponds to an increase in pressure.

"This discovery remained unnoticed. In the first instance the facts were only required for the elucidation of certain physiological questions.

"And it was not until 1886 that Van 't Hoff developed a theory of solution based on these phenomena.

"Osmotic pressure is a specific property of the substance in solution, and in this respect resembles gaseous pressure. The analogy between the state of solution and the gaseous state is clearly shown (pp. 115–17). Dissolved substances exert the same pressure in the form of osmotic pressure as they would exert if they were gasified at the same temperature without change of volume.

"All that we know of gases holds good for solutions, substituting osmotic for gaseous pressure.

"Osmotic pressure is, in some instances, very great."

And it seems clear that osmotic pressure is not a mythical, but a real or actual force of considerable power, and one which may be rationally applied to the elucidation of the cause of the carbonization of iron; further, it may even afford a clue to the phenomena observed in the production of other alloys.

As regards the carburization of iron, the physical theory of solution, "founded on the identity of osmotic with gaseous pressure," seems the only one capable of affording a satisfactory explanation of the facility with which carbon combines with iron.

The chemical, or old, theory of solution apparently fails to do this. The same may be said of the assumption that chemical combinations of iron and carbon are formed. Although it must be granted such combinations may exist, yet, in the author's opinion, complete proof is still wanting. It is really difficult to realize, when dealing with stable bodies like iron and carbon, how their union can be thus accomplished.

On the contrary, the application of the law of osmose renders the conception of the transfer of carbon to iron very easy. This force, exerting probably almost illimitable power in nature, seems the only one capable of overcoming the *inertia* of bodies; such, for instance, as that of iron and carbon.

The physical theory of solution has hitherto only presumptively herein been applied to the solution of solids in liquids; and it may be asked, Is it applicable to the case of the solution of solids in solids, such as carbon and iron, when heated?

To this one can reply with confidence that the absolute solid has no existence. Unless we reject the atomic theory, it is evident that no tangible mass of matter can be termed a solid: it is an agglomeration of atoms. Further, accepting the definition of what is termed the atomic volume—*i.e.* the space occupied or kept free from the access of other matter, by the material atom itself, together with its investing sphere of heat—it follows that the atoms must be apart from each other in the so-called solid mass, and the distances between the atoms are probably considerable as compared with the actual volume or size of the atoms themselves. Therefore, there can be no difficulty in conceiving that osmotic pressure plays a part in the case of a mass of matter, "conventionally termed a solid." It is only a question of degree; the quantity of matter dissolved in a given time is simply a function of the temperature applied, and at a low temperature, the effective osmotic pressure in the case of solids seems comparable to that of a liquid evaporating under pressure of its own vapour. Evaporation is retarded, and the analogy may hold good in the case of the conventional solid.

JOHN PARRY.

PHOTOMETRIC OBSERVATIONS OF THE SUN AND SKY.[1]

ATTEMPTS have been made by Clausius and various other mathematicians to calculate the light at different points of the perfectly clear sky, and to compare the light of the whole (or a portion) of the sky with that of the sun. The difficulties of photometric measurement have prevented any of the theories being thoroughly established by experimental verifications.

In the first period of photography, it became a matter of practical importance to have some way of testing roughly the "actinic activity of diffused daylight," in order to obtain a guide for the time of exposure. Very many photographers, in those days when the evils of over-exposure could not be corrected in the printing, must have exposed a scrap of sensitive paper, and thence concluded how many seconds' exposure they would allow.

[1] Ostwald, "On Solution."

[1] "Photometric Observations of the Sun and Sky," by Wm. Brennand. Proceedings of the Royal Society, vol. 49, n. 288, April 18, 1891, pp. 255–280.

From this point it would be a very easy step to test the "actinometric effect on sensitized paper" ("chemical action" of Roscoe) of different skies, or of the sun at different altitudes. It is not probable that the chemical action is simply proportional to the light; but it would be soon found that the "chemical action" could be much more accurately measured than the light.

Sir Henry Roscoe (partly in junction with Bunsen and with Thorpe) made many investigations and various publications between 1859-70 on the chemical action of the sun and sky as measured by its effect in darkening photographically sensitized paper. Roscoe delivered the Bakerian Lecture in 1865, "On a Method of Meteorological Registration of the Chemical Action of Total Daylight."

Throughout his investigations Roscoe pursued a direct method of experiment: he elaborately investigated a method for obtaining always paper of standard sensibility; he devised a plan for obtaining a light of standard intensity; he then exposed a piece of the paper to the action of the sky, or of sun and sky, or of a portion of the sky, and compared the effect produced in a given number of seconds with that produced in the same paper in the same number of seconds by his standard light. Roscoe also, by a laborious method, verified his fundamental asumption that light of intensity 50 acting for 1 second has the same effect as light of intensity 1 acting for 50 seconds.

Roscoe took half-hourly readings at Manchester, and thence gave the (comparative) actinic effects of the sky at different seasons of the year. Also he compared the chemical intensity of total daylight at Kew and Para, and investigated the relation between the sun's altitude and the chemical intensity of total daylight in a cloudless sky. By total daylight Roscoe meant the chemical action produced by the sun and whole sky together on a piece of paper exposed horizontally.

Roscoe found that his readings were enormously affected by the cloud-haze or invisible vapour in the air in England; he got his results, as to comparison of the chemical intensity at different seasons of the year, and at different altitudes of the sun, by assuming that in the average of a large number of observations, the effects of cloud, &c., would be self-destructive.

Roscoe found that the "chemical effect" of the sun depended only on his altitude (in a cloudless sky), being the same at Para and at Kew. He got very anomalous results as to the effects in spring and autumn in England, probably because the effects of cloudiness were not self-destructive in his series of observations. He arrived, by "averaging" the cloud irregularities, at the law that "the relation between the sun's altitude and the chemical intensity of total daylight is graphically represented by a right line" (a result only a rough first approximation to the truth). Roscoe obtained small result in comparing the chemical action at different points of the same sky, partly because he could make no experiments in person on a tropical clear sky, partly because to note these differences requires superior instruments to the direct experiment method alone tried by Roscoe.

Mr. W. Brennand was engaged at Dacca in observations, parallel to those of Roscoe, and nearly contemporaneous, 1861-66. Brennand was quite unaware of Roscoe's experiments. Being an amateur photographer, and his own photographic chemist, he was first led to devise an instrument for testing the chemical action of sun and sky, in order to obtain guidance for the number of seconds to expose a photographic plate. He was soon led on to investigate the effect of the sun at different altitudes, the effect of the sky for different altitudes of the sun, and finally the law of distribution of the "chemical action" in a perfectly cloudless sky.

Brennand's procedure in experiment differed fundamentally rom Roscoe's in two points:—

(1) Brennand only attempted observation in the cold weather at Dacca when he had a complete horizon of clear sky. He was thus enabled to carry his investigations into the laws of chemical action in a cloudless sky much farther than Roscoe, 90 per cent. (at least) of whose observations were obscured by cloud irregularities that could not be allowed for.

(2) Instead of Roscoe's direct method of observation, Brennand was early led to devise an instrument (the water-motion actinometer (see NATURE, January 8, 1891, p. 237), by the aid of which he was independent both of the standard light and standard paper attained by Roscoe with so great labour. The sun himself was, in fact, Brennand's standard light, and the darkening of each paper was read as a ratio; for instance, if an exposure of 10 seconds to sun and sky produced the same tint in the paper that was produced by the sun alone in 17 seconds, then the effect of the sun alone was reckoned $\frac{10}{17}$ of the sun and sky together. It is clear that *any* uniform paper should give such ratios the same, though the actual shades produced would be different in different papers. All the papers made by Brennand himself were found "uniform," *i.e.* to within the limits of variation (say, 2 per cent.) within which the darkened paper can be read, *i.e.* the shades can be matched. Any good photographic paper is found uniform enough for the purpose; but some of the ordinary photographic papers tried lately in England have been found not good enough; the nature of the irregularities introduced by imperfect paper is such as to suggest very soon their cause.

It is to be noticed that all that can be observed is a ratio: the observations in Roscoe's direct process are not absolute. In that process there is a standard unit, viz. the blackness produced in the standard paper by the standard light action at the unit of distance for n seconds. Any other light that produces this blackness has the numerical value $\frac{x}{n}$ in Roscoe's unit. There is little doubt but that Roscoe got his standard light and standard paper, each time he recovered them, correctly within the percentage of error involved in the reading. He would be certain to have prepared his salts of exactly the proper strength; but there is an element of uncertainty in the degree in which papers apparently of similar texture and in a similar state of dryness, &c., take up salts. This element of uncertainty is avoided by Brennand's method, which is far more absolute than Roscoe's.

The water-motion actinometer gave Brennand, for each observation, a shaded strip darkened gradually from 0 to 8 (or to 16) seconds. He could note on this the point at which a particular unit of darkening was produced, and the inverse of this time gave him a measure of the ratio of the observed "chemical action" to that which had produced the unit darkening.

This, of course, involved the assumption that light of intensity 50 acting for 1 second has the same effect as light of intensity 1 acting for 50 seconds. This Brennand thought might be assumed; but he proved it in the following very simple manner.

A slip of sensitized paper is formed into a ring (a short cylinder) and placed round a light (the wick of a candle was used, but any light would do, irregular or not) excentrically. After a certain time the slip is examined and found to be shaded gradually from the farthest to the nearest point, the effect at each point varying inversely as the square of the distance.

Thus if A be the source of light, O the centre of the ring, and if we have OB $= a$, OA $= b$, POB $= \theta$, we shall have the chemical effect at any point P of the slip vary as

$$\frac{1}{R^2} = \frac{1}{AP^2} = \frac{1}{a^2 + b^2 - 2ab\cos\theta}.$$

In a particular experiment Brennand took $a = 1{\cdot}4$ inch, $b = {\cdot}4$ inch.

$$\cos^2\frac{\theta}{2} = \frac{3{\cdot}24 - R^2}{2{\cdot}24}.$$

Taking the unit of intensity that at the distance 1 inch from the wick, and calculating the values of θ for values of the intensity 1, ·75, ·5, and ·3, we have $\theta = 0$, $20^\circ\ 10'$, $78^\circ\ 34'$, and $141^\circ\ 48'$ respectively. The lengths of arc corresponding to these are found to be ·49 inch, 1·92 inch, and 3·45 inches respectively. These lengths can be marked off on the slip. Another slip can then be darkened in the water-motion actinometer, by any light; a unit can be marked on this slip at the point where the shade corresponds with that at the unit in the ring slip; it then can be seen whether the intensity of shade at the distance ·49 on the ring slip agrees with that at three-fourths the time for unit on the actinometer slip; and similarly for the other calculated values. This experiment verifies the law assumed, and moreover affords a check on the paper employed, and on the closeness with which tints can be matched.

Another important means of verification was employed by Brennand, which Roscoe does not appear to have availed himself of. Calling the effect of the sky alone in darkening paper B, and the effect of the sun and sky together A, Roscoe observed A and observed B, and then calculated the effect of the sun alone as A − B. Brennand did this; but also observed the sun alone by the simple device of a vertical slit in a shutter, and was thus able to check the accuracy of his method and of his work.

Having thus established the trustworthiness of his *modus*

operandi, Brennand, with his water-motion actinometer, drew up, by an ample series of observations extended over several years, the Table B given in NATURE (January 8, 1891, p. 237). The numbers in this table are ratios, and they may be all multiplied by any number without any real alteration in the table. The unit of chemical action originally started with was the blackness produced by 100 grains of a candle burnt at the unit of distance; and this is the unit which underlies Table B. Brennand early found, as Roscoe found, that the sun has always the same effect at the same altitude in a perfectly clear sky. Hence, in all the later observations the unit was recovered from the sun.

Thus, to take a series of observations, with the water-motion actinometer, with strips of an unknown (but uniform) paper: first, a strip is placed in the instrument, the sun alone being admitted by the vertical slit, and the sliding shutter is run up; we thus get a gradually tinted slip beside the gauge marked in seconds. The altitude of the sun is noted; suppose it $30°$; the number in Table B for this altitude is 0·1070, *i.e.* the number of seconds which produces unit darkening by sun alone at this altitude is $\frac{1}{0·107}$ seconds $= 9\frac{1}{3}$ seconds. Then on the sun strip a mark is made opposite the $9\frac{1}{3}$ seconds graduation on the gauge; this is the unit blackness for the paper, and any subsequent strip exposed is "read" by marking the point on it which has the same blackness.

This method of recovering the unit is not sufficient to determine, for instance, whether the sky in England on a certain morning was really clear, *i.e.* as truly clear as the Dacca cold-weather sky. To determine this particular point, Brennand lately in England burnt 100 grains of a candle (as near as he could get) similar in composition to his Dacca candle, and the result shows conclusively, by the exact accord of several observations lately made near Taunton with corresponding old observations at Dacca, that in this case the two candles must have produced equal effects. But it is obvious that the candle could only be trusted by these results. The experiments made with the candle were not made to recover the Dacca unit, but to test the candle. The exact agreement in the several results raises the very strongest presumption that the Taunton candle was equal to the Dacca candle. It is, however, possible that the Taunton sky varied for the several observations in exact ratio with a variation in the Taunton candle; and it can only be said that Brennand's observations on this particular point, so far as they go, support Roscoe's result that the chemical action of the sun is the same at the same altitude in a perfectly clear sky, always and everywhere.

When the chemical action of the sky (or of some portion of it) on a piece of flat paper is observed, what is measured is the integral of the resolved effects of each sky element. Thus if, as in Roscoe's experiments, the piece of sensitized paper is exposed horizontally, and the effect of the whole sky (the sun being stopped off) is taken, we have the total effect of a ring of the sky distant θ from the zenith to be multiplied by $\cos\theta$, and then the effect of all such rings from the zenith to the horizon to be summed. This view of the resultant action suggested to Brennand the more original branch of his investigations. He was early led to suspect that the chemical action of the sky varied in different parts of it. He devised an instrument, which he calls the mitrailleuse actinometer, by which he was enabled to prove that the chemical action of the sky is a minimum in the great circle distant $90°$ from the sun; for an altitude α of the sun this minimum he calls i_α. Brennand then further proves that the chemical action (at the same time) at any other point of the sky distant θ from the sun is then $i_\alpha \operatorname{cosec}\theta$.

Having established these important laws, Brennand is able, by mathematical process, having i_α given him, to calculate the total effect of any defined portion of the sky on a plane of sensitized paper exposed at any given angle. He was thus enabled to compare Roscoe's readings of total diffused daylight on paper exposed horizontally with his own Dacca readings on paper exposed perpendicularly to the direction of the sun.

These investigations led Brennand to a theoretic value for the duration of twilight, and to the devising a new instrument, the "octant actinometer," by which the fundamental constant i_α can be observed directly.

This "octant" actinometer observes one-eighth of the heavens, cut out by three planes at right angles to each other, placed so that the line OS, the intersection of two of the planes, passes through the sun. Owing to the cloudy skies of Taunton, Brennand (who experiments only with a clear sky) had been able very imperfectly to test this instrument at the time his paper was read to the Royal Society. He has since found that this "octant" may be turned in any way round OS (above the horizon, of course) without altering the reading on either of the three planes of the octant. This "octant," therefore, only requires one-fourth of the visible hemisphere round the zenith to be clear, for a good observation. What is more important, it enables the observer, when the sky is clear, and the sun's altitude from $30°$ to $60°$, to take an observation of a part of the sky entirely $30°$ from the horizon; so that the uncertainty arising from haze near the horizon (which could not before be allowed for) may by this capital instrument be avoided, and i_α obtained without any integrations or calculations beyond division by a number.

In the whole of these later developments, Brennand's work is entirely original. Sir Henry Roscoe, following a somewhat different course of inquiry, has made experiments on the chemical action of the sun and sky at different levels above the sea; and on the total effect during different months or seasons of the English sky with all its cloud, fog, and smoke; which last is an important practical measure of the climate in its influence on vegetation, and perhaps on human health.

The researches of Roscoe and Brennand have thus, though overlapping at particular points, extended mainly in different directions. Brennand, in the ground covered by both, puts forward far the more accurate determinations; his table (B) given in NATURE, January 8, 1891, p. 237, professes to be of the same character and value as a table of the constants of refraction,—Brennand has had half a century's experience with the chemistry of photographic paper, and is an excellent mathematician of the old school. Moreover, the three leading actinometric instruments he has devised, the water-motion, the mitrailleuse, and the octant, show him to be possessed of much resource in devising instruments of research.

INORGANIC SYNTHESIS OF AZOIMIDE, N_3H.

A METHOD of synthesizing this interesting compound of nitrogen and hydrogen, by means of a simple reaction involving only purely inorganic substances, has been discovered by Prof. Wislicenus, and is described by him in a communication to the current number (No. 12) of the *Berichte* of the German Chemical Society. The reactions by which azoimide has hitherto been obtained have all been of an organic nature, and more or less complicated. The mode of preparation described by its original discoverer, Prof. Curtius, in reality depends upon a very simple reaction, that of nitrous acid upon hydrazine, N_2H_4, the other hydride of nitrogen whose preparation we also owe to Prof. Curtius,

$$\begin{array}{c} NH_2 \\ | \\ NH_2 \end{array} + HNO_2 = N_3H + 2H_2O.$$

Hydrazine, however, has only yet been prepared from its organic derivatives, and moreover it has not been found practicable to actually convert free hydrazine itself by means of nitrous acid into azoimide, only certain organic derivatives being acted upon by nitrous acid with production of azoimide. The perfected mode of preparation described by Prof. Curtius at the close of last year is very briefly as follows. Benzoyl hydrazine, $C_6H_5 . CO . NH . NH_2$, is first formed by reacting with ethyl benzoate upon hydrazine hydrate:

$$C_6H_5 . COOC_2H_5 + N_2H_4 . H_2O \\ = C_6H_5 . CO . NH . NH_2 + C_2H_5OH + H_2O.$$

The benzoyl hydrazine is then converted by means of nitrous acid, obtained from a mixture of glacial acetic acid and sodium nitrite, into the benzoyl derivative of azoimide:

$$C_6H_5 . CO . NH . NH_2 + HNO_2 = C_6H_5 . CO . N\!\left\langle \begin{array}{c} N \\ \| \\ N \end{array} \right. + 2H_2O.$$

From benzoyl azoimide the sodium salt of azoimide is next formed by treatment with sodium ethylate:—

$$C_6H_5 . CO . N\!\left\langle \begin{array}{c} N \\ \| \\ N \end{array} \right. + C_2H_5ONa \\ = C_6H_5 . COOC_2H_5 + Na - N\!\left\langle \begin{array}{c} N \\ \| \\ N \end{array} \right. .$$

Free azoimide is finally obtained by distilling the crystals of the sodium salt with dilute sulphuric acid, and repeatedly re-distilling over fused calcium chloride the hydrated liquid which first passes over.

As an alternative method which has some advantages as regards facility of manipulation, Prof. Curtius employs the hippuryl derivative of hydrazine instead of the benzoyl compound The product of the action of nitrous acid upon this compound is a substance which can be readily isolated in crystals, and if these crystals are dissolved in dilute caustic soda, the solution at once yields azoimide upon distillation with dilute sulphuric acid.

Before describing the inorganic synthesis of Prof. Wislicenus, it may be mentioned that a still simpler organic synthesis of azoimide from the long known diazobenzene imide, $C_6H_5N_3$, has been achieved by Drs. Noelting and Grandmougin. Although diazobenzene imide itself is too stable a substance to yield azoimide directly by simple saponification with soda, these chemists found that its dinitro derivative yielded directly to the attack of an alcoholic solution of potash, the potassium salt of azoimide being formed, which of course gave free azoimide upon distillation with dilute sulphuric acid.

The inorganic synthesis of azoimide now achieved by Prof. Wislicenus depends upon the interaction of ammonia gas and nitrous oxide in the presence of heated metallic sodium. Ammonia and nitrous oxide do not act directly upon each other, not even when a mixture of the two gases is passed over caustic bases—soda-lime for instance. But they react readily in presence of metallic sodium. The explanation of this lies in the fact that the sodium amide discovered by Gay Lussac and Thenard is first formed, and this compound reacts with the nitrous oxide with production of the sodium salt of azoimide:—

$$NaNH_2 + N_2O = NaN_3 + H_2O.$$

The water produced at the same time reacts with one-half of the sodamide, forming caustic soda and liberating ammonia gas:—

$$NaNH_2 + H_2O = NaOH + NH_3.$$

Hence the complete reaction may be expressed by the equation—

$$2NaNH_2 + N_2O = NaN_3 + NaOH + NH_3.$$

As the sodium salt is less explosive than most of the other salts of azoimide, requiring a higher temperature and not being sensitive to percussion, the experiment is not dangerous if proper care is exercised, and even if local explosions do occur they have not yet been observed to shatter the glass tube. Unfortunately glass is somewhat strongly attacked during the reaction, but if iron tubes are employed the reaction is not so completely under control.

In actually conducting the experiment, metallic sodium, in pieces not exceeding half a gram in weight, is placed in a series of large porcelain boats, which are then laid in a glass combustion tube, from which the air is subsequently displaced by means of a current of ammonia gas. The tube is heated carefully in a combustion furnace, when the sodium fuses and gradually passes into sodamide. When all the metal has been thus changed, the stream of ammonia is replaced by one of dry nitrous oxide. The temperature should now be lowered to between 150° and 250°, and for this purpose Prof. Wislicenus surrounds it by an iron explosion chamber, which forms a capital air-bath, the temperature of which can be regulated by observing a thermometer or thermometers inserted in it. The sodamide now slowly increases in bulk and becomes converted into the sodium salt of azoimide. As soon as ammonia ceases to be carried away in the stream of issuing nitrous oxide the reaction is completed. Upon cooling the sodium salt is found as a porous pumice-like substance, much distended by the escaping ammonia.

The sodium salt of azoimide is also formed when ammonia and nitrous oxide gases are simultaneously passed over melted sodium; the yield, however, is not so large, and there is danger of the sodium inflaming in the nitrous oxide.

The fact that the sodium compound obtained is the sodium salt of azoimide has been proved both by direct analysis (a determination of nitrogen yielding close upon the theoretical amount) and by its properties. The product of the reaction on being removed from the combustion tube was thrown into water, and the filtered solution distilled with dilute sulphuric acid. The distillate possessed the intolerable odour characteristic of azoimide, and behaved exactly like a solution of that substance in water. It gave precipitates with nitrates of silver, mercurous mercury and lead, which when separated and dried were found to possess all the properties of the silver, mercury, and lead salts of azoimide respectively. The fact that these salts were those of azoimide was indeed sufficiently apparent from their violently explosive nature, and the characteristic flames which were produced during their explosion. Moreover, gold dust was rapidly dissolved with production of the red solution described by Prof. Curtius.

A quantity of the silver salt was subjected to analysis, and was found to contain 71·7 per cent. of silver, the amount calculated for AgN_3, being 71·8.

Instead of sodium, either potassium or zinc may be employed. Potassium answers almost as well as sodium, forming first an amide when heated in a current of ammonia, which is subsequently converted by nitrous oxide into the potassium salt of azoimide. Zinc likewise behaves in a similar manner, but the yield of the zinc salt of azoimide, ZnN_6, is not so good as in the cases of sodium and potassium. To a greater or less extent, therefore, it would appear that metallic amides when heated in a current of nitrous oxide are generally converted into salts of azoimide. The alkali metals, however, appear to be best suited for practical use.

A. E. TUTTON.

THE REPORTED VOLCANIC ERUPTION AT GREAT SANGIR.

ACCORDING to a Reuter's telegram from Sydney, despatched on July 17, the vessel *Catterthun*, belonging to the Eastern and Australasian Steamship Company, which had arrived at Sydney from China, brought a report of a terrible disaster in the vicinity of the Philippine Islands. She called on her voyage at one of the chief ports of the island of Timor, where rumours had been received according to which the island of Sangir, situated between Celebes and Mindanao, had been destroyed by a volcanic eruption. The whole population, numbering 12,000, was reported to have perished. The captain of the *Catterthun* stated that on the voyage his vessel passed through some miles of volcanic *débris*.

We may not for some time receive further details as to the real extent of the disaster reported by the captain of the *Catterthun*, but in the meantime the following account, by Mr. Sydney J. Hickson, author of "A Naturalist in North Celebes," of the island and of the history of its volcanic energy—which appeared in the *Times* of Tuesday—will be read with interest:—

Sangir, or "Great Sangir," as it is more frequently called by the natives of the Archipelago, is the largest of a chain of volcanic islands that connects the northern peninsula of Celebes with the southern point of the island of Mindanao. The islands, rising abruptly from the floor of the very deep Celebes sea—a depth of over 2,000 fathoms was found by Her Majesty's ship *Challenger* quite close to Great Sangir—are very mountainous and covered by dense tropical forests.

The islands Ruang and Siauw are both little more than volcanoes standing in the sea, but Sangir is a large island 25 miles long by about 15 miles broad, with undulating hills and valleys occupying its southern half, and the great Awu volcano and its slopes the greater part of its northern half.

When I visited the islands in November, 1885, the Ruang and the Awu were quiet, but the Siauw was sending out dense volumes of smoke that varied little in intensity from day to day.

From the accounts I received from the natives and from the records of the islands in the Dutch books of travel, it seems that the Siauw volcano has never been very violently active, but both the Ruang and the Awu have a history full of most terrible and heart-rending episodes. Of the Ruang I need not say much. The last serious eruption occurred in 1871, when at least 400 persons lost their lives either by the sudden rise of the sea water that accompanied the eruption, or by the showers of stones and ash. Of the Awu volcano we find recorded in Valentijn's "Oud en Nieuw Oost Indien" that a most terrible eruption occurred which lasted from the 10th to the 16th of December, 1711. Sjamsialam and his son, the Princess Lorolabo and her daughter Sarabanong, and over 2000 people of the kingdom of Kandahar were killed. On March 2, 1856, there was another fearful eruption, which lasted until March 17, and destroyed nearly 3000 human lives. The streams of boiling water and of steam which poured down the mountain slopes rather than the flow of lava caused the enormous mortality of this second eruption. After the eruption of 1711 it seems that a large lake of water was formed in the crater, and a certain privileged class of Sangirese were allowed by the gods to visit this lake every three or four months to test the water with their rice. If

the water was hot enough to cook their rice they took it for a sign that an eruption would shortly follow. The great eruption came in 1856. The waters of the lake began to boil, burst their banks, and flowed down the sides of the mountain towards Tabukan and Taruna, causing immense destruction of human lives and property.

Concerning the present eruption we learn very little at present, but it seems to me very improbable that the whole island has been destroyed, and, from the sparseness of the population on the slopes of the Awu, it is also very improbable that so many as 12,000 persons have lost their lives.

The population consists of one Dutch Controlleur, who may possibly be married, some three or four German missionaries with their wives and children, one or two European traders, a few Chinamen, and the remainder Sangirese Malays. The island is governed by five native Rajahs, who are advised by the resident Dutch Controlleur. For many years there has been no war or other disturbance, but the island, notwithstanding the richness of its soil, is not in a very prosperous condition. The only produce of any importance is copra, but some good ebony and other timber is found in the forests that cover the islands.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 11.—M. d'Abbadie in the chair.—On a slight additive correction which may have to be applied to the heights of water indicated by sea-gauges when the swelling or choppy agitation of the sea attains a great intensity; case of a swell, by M. J. Boussinesq. From theoretical considerations and practical experiments it appears that a tide-gauge exposed in a lateral basin will not give correct indications of level for a choppy sea, but that it will register a lower level than it would if the water were at rest. For a wave 1 metre high the difference may amount to 1 cm.—On the determination of the density of gases, by MM. Henri Moissan and Henri Gautier. This is achieved by a new method, which makes it possible to determine the density within one or even one-half per cent. from a volume of 100 cc. of the gas. The principle is analogous to that adopted by Dumas in his researches on vapour-densities, and consists in measuring the difference between the weight of a known volume of the gas and an equal volume of air at the same temperature and pressure. If this difference in grammes be denoted by p, and if v denote the volume of the gas or air at temperature t° and pressure H, the density is given by the equation

$$p = v \times 0{\cdot}001293\,(x-1) \times \frac{H}{760} \times \frac{1}{1+0{\cdot}00367t}.$$

The apparatus consists of a glass cylinder of about 90 cc. capacity connected at its lower end with a glass tube leading through an india-rubber tube to a movable flask filled with mercury, by means of which the pressure inside the measuring cylinder can be regulated. The latter is surmounted at its upper end by a weighing bulb separated by a three-way cock, by which communication can be established with a fine bent tube. In the experiment, the bulb is first exhausted, then filled with dry air and again exhausted, this being repeated about ten times. It is then shut off, and the fine tube and the measuring cylinder are filled with mercury by lifting the reservoir. The capillary tube can now be used as a pipette, and the gas is drawn into the cylinder and allowed to assume a constant temperature during the night at the pressure of the atmosphere. The bulb is then exhausted and placed in communication with the cylinder, and all the gas is driven into the bulb by raising the mercury flask. The bulb is then carefully removed, and dry air is allowed to enter so as to bring the pressure nearly up to that of the atmosphere. Lastly, the bulb is placed on the balance; the weight which has to be added or removed to obtain equilibrium represents p, which, substituted in the above equation, gives the density. The specimen of gas operated upon can be subsequently used for other experiments.—On the order of appearance of the first vessels in the flowers of some *Lactucæ*, by M. A. Trécul.—On the effects of cold and drought on this year's harvests, and the means by which it has been attempted to combat the evil, by M. Chambrelent.—On the alcoylcyanocamphors and the benzine-azocamphocarbonic ethers, by M. A. Haller.—On the *Libytherium maurusium*, a great Ruminant of the plaisancian pliocene formation of Algiers, by M. A. Pomel.—Measurement of the absolute intensity of gravity at Breteuil (International Office of Weights and Measures), by M. G. Defforges. This was carried out by means of two reversible pendulums constructed by the brothers Brunner, one being 1m., the other 0·5m. long between the knife-edges. The oscillations took place in air and in a vacuum, the latter being continued for 12 to 24, and once for 50 hours. The results were:—

For the length of the seconds pendulum...... 0·993952
For g.. 9·80991

—Photographs of the chromosphere, the prominences, and the solar faculæ, taken at the Kenwood Astro-physical Observatory, Chicago, by M. E. Hale.—On the practical calculation of the dimensions of the outflow orifices of saturated vapour into the atmosphere, under constant or varying conditions; application to safety valves, by M. H. Parenty.—On a chloro-nitrogen salt of palladium, by M. M. Vèzes.—Double chlorides formed by lithium chloride and the chlorides of the magnesium series, by M. A. Chassevant.—Researches on nickel and cobalt, by MM. Ch. Lepierre and M. Lachaud.—On the iodomethylates of quinine, by M. E. Grimaux.—On the camphocarbonic methyl ethers, methyl camphor, and some nitrogen derivatives of cyanocamphor, by M. J. Minguin.—Action of the metalloid nitrides and hydronitrides on the oxyhydrocarbon compounds, by M. R. Vidal.—On some ferruginous medicines, by M. H. Le Chatelier.—Contributions to the history of mineral waters; on the alumina contained in these waters, by M. F. Parmentier.—The respiratory value of hæmocyanine, by M. L. Cuénot.—Physiological action of spermine; interpretation of its effects on the organism, by M. Alexandre Poehl.—On the embryonic circulation in the head of the axolotl, by M. F. Houssay.—On the *Belisarius Viguieri*, a new fresh-water copepod, by M. Maupas.—On the evolution of the embryo of a fowl submitted during incubation to a continuous rotation, by M. Dareste.—The boghead of Autun, by MM. C. Eg. Bertrand and B. Renault.—On the constitution of the fructifying ears of *Sphenophyllum cuneifolium*, by M. R. Zeiller.—A review of the geological constitution of the regions situated between Bembé and Crampel Peak (Congo), after specimens collected by M. Jean Dybowski.

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